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INTROSPECTION IN DEMENTIA PRECOX

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Introspection is generally acknowledged to be, if not the most important, at least one of the fundamental methods of psychology. The psychopathologist, however, in his study of the abnormal mind, has availed himself very little of the introspective method; and, in those cases in which he has done so, his use of the method has been so nearly unconscious and so correspondingly uncritical, that the results have been of very much less psychological value than those results which a refined application might have given. It is true that the psychiatrist depends largely, in a mental diagnosis, upon the verbal statement of the subject about his experiences; and the psychopathologist in a mental test, such as a comparison of lifted weights, also depends upon the verbal statement of the subject as an expression of mental facts; but such statements cannot be called 'introspection' in the proper sense of the word. They indicate facts about mind, but they do not, as a rule, directly describe mind. The material is information[1] and it is usually information uncritically accepted. As such it may prove adequate for a symptomatology, but it can hardly in itself lead to an accurate psychology.

The use of introspection in a study of association in the insane has been championed by Ley and Menzerath,[2] who write:

[1] The term, 'information,' is used throughout this paper in the meaning of *Kundgabe, i.e.,* statements about mind, but not directly describing mind.

[2] *L'Étude expérimentale de l'association des idées dans les maladies mentales,* 1911, 32.

Les premiers auteurs qui s'occupèrent de l'étude expérimentale des associations ont cru pouvoir s'en tenir en côté purement objectif; ils comparient tout simplement les réactions aux mots inducteurs et en recherchaient les liens logiques.....Ces interprétations *logiques* devaient necessairment aboutir à un échec, puisqu'elles négligeaient le côté le plus fondamental du phénomène, c'est-à-dire, le côté subjectif, *psychologique*. C'est à Alfred Binet que revient l'honneur d'avoir rehabilité le facteur subjectif en psychologie expérimentale, en y introduisant l'*introspection*.....

Nous avons personellement constaté, et nos protocoles donnent de nombreux exemples à ce sujet, que bon nombre d'associations resteraient tout à fait inexplicables, si nous n'avions, pour éclairer leur genèse, l'introspection du sujet.

These writers report experiments performed with nine classes of insanity,[3] in all of which they obtained introspections from the subjects. The introspections consist, however, of the statement of the meaning of the reaction word, of its connection with the stimulus word, or of some such simple explanation. At best the investigators have done no more than to substitute a logical interpretation by the subject for a logical interpretation by the experimenter. They have made no attempt to induce the subject to describe consciousness. He merely indicates meanings, and in doing so he is not introspecting (if we limit the use of the term introspection to a direct description of the contents of consciousness) but giving information.

Franz has recognized the availability of the introspective method in psychological work with the insane. In speaking of the difficulty of this sort of work, he writes:

The question has often been asked me whether or not the insane are more difficult to work with than normal people. I believe both to be equally easy and equally difficult subjects. Many insane do not introspect well, but few normal people do so. Some insane are more introspective than the average normal individual,—and from some no introspections can be obtained. It is not necessary to work with the patients who introspect badly or not at all; there is a possibility of a selection of subjects just as there is in normal psychological work. In general I think there is no inherent difficulty in investigating the mental conditions of the insane.[4]

The trend of psychopathology does not seem, however, to have led toward an employment of the introspective method. The writer is not aware of any paper, other than the one mentioned, in which introspections have been systematically

[3] (1) Dementia precox, (2) manic-depressive psychoses, (3) toxic excitement, (4) neurasthenia and psychasthenia, (5) hysteria, (6) nerve traumatism, (7) paranoia, (8) general paralysis, and (9) sleeping sickness.

[4] *Psychological Opportunity in Psychiatry, Jour. Philos. Psych. and Sci. Meth.*, 3, 1906. 567.

recorded. This state of affairs is probably due to a lack of trust, on the part of the investigator, in the reliability of the statements of the insane; and such scepticism is not unwarranted. In introspective studies with normal subjects, we are inclined to conjure with the term "trained observer," until the novice in the science might infer that all psychological facts are based on the experience of a few, specially educated individuals; yet even with the best observers the reliability of the method has been questioned,[5] and recent studies have sought more clearly to define the nature of introspection and more exactly to determine its limitations.[6] It is no wonder, when the validity of the report of a normal observer with psychological knowledge and perhaps years of introspective training is questioned, that there is little thought of applying the method to untrained observers of abnormal mentality. There is, however, an error in the sceptical position, an error which is both logical and empirical. The fact that reports of complex experiences by trained persons may sometimes be unreliable does not mean that the reports of simple experiences by untrained persons may not be reliable; and no rule is completely established until it is found operative in experience. Accordingly, it is proposed in this paper to examine critically the actual introspective reports made by subjects in one form of insanity,—dementia precox,—and to determine, as far as possible, the reliability of these reports.

The introspections which will be considered are those given for the course of consciousness during the learning of a pencil-maze by eight subjects with dementia precox. The writer has already published the protocols in a recent study,[7] where they are printed fully in order that the reader may determine for himself what degree of reliability he will accord them. The writer was unable at the time of that publication to discuss the introspections critically, but promised that such a discussion should be forthcoming, as soon as additional experimental

[5] *Cf.*, Dodge, R., *The Theory and Limitations of Introspection, Am. Jour. Psych.*, 23, 1912, 214 ff.

[6] Anschütz, G., *Ueber die Methoden der Psychologie, Arch. f. d. ges. Psych.*, 20, 1911, 414 ff.
Titchener, E. B., *Prolegomena to a Study of Introspection, Am. Jour. Psych.*, 23, 1912, 427 ff.; *The Schema of Introspection, ibid.*, 485 ff.

[7] *Learning in Dementia precox, Psych. Rev. Monog. Series, Vol. 15, No. 2,* 1913. After this article was in the hands of the printers, it was found that an unavoidable delay in the publishing of the monograph would necessitate the appearance of this supplementary paper earlier than the appearance of the study upon which it is based. The latter should, however, be available shortly.

work in the way of control series had been performed. The present paper aims to fulfill that promise.

The Criteria of Reliability

We must here inquire what the criteria of the reliability of a description of consciousness are. We must establish our critical method before we can proceed to the criticism.

In the first place, we may assume that the object of a descriptive psychology is to establish definite, permanent, concise descriptions of consciousness,—the facts of psychology,—which may be based upon the verbal introspective reports of trained observers, upon information of untrained observers, upon experimental graphic records, or upon casual observation, either introspective or informational,—as given verbally by a present observer, as recorded in literature, or as indicated by such products of mind as art, language, and customs. We shall concern ourselves here with such descriptions as are based upon verbal reports of observers,—thus including trained introspection and information, between which it is not easy, at present, to draw a sharp line of demarcation.

The availability of these bases for a description of consciousness depends upon the ease and reliability with which they can be interpreted accurately as describing consciousness. Even in the case of introspection by the trained observer, which is in itself a direct description of consciousness, a certain amount of interpretation must take place; for, in order that the introspection may become a part of a systematic, descriptive psychology, the psychologist must select from the protocols, and bring the verbal expression of the observer into accord with the terminology of the science. A description, then, as a unit of a systematic psychology, is always liable to error arising from a misinterpretation of the meaning of the observer, as expressed in his report.

A more frequent source of error arises from the fact that the report of an observer, in the meaning intended by him, may still not describe the actual experience exactly as it occurred. Even observers with the best training may report factors that were not actually present in the experience, and it is one of the commonplaces of the psychological laboratory that no report ever enumerates all of the factors present in the consciousness described.

In accordance with these two possible sources of error,—the intelligibility and the accuracy of the report,—we shall divide our method of criticism. The rubrics which we shall name may not be logically exhaustive. They emphasize, however,

those factors which are important as affecting reports by untrained observers; and in such form they should be approximately exhaustive in fact for the cases considered,—at least as far as can be determined at present.

I. Intelligibility of Report

The adequacy of a verbal report as an accurate description of the consciousness described depends upon the intelligibility of the report. The psychologist must interpret the language of the observer, and only to such extent as he attaches to the language of the report the meaning that was intended by the observer will the final conclusion represent the facts. We may distinguish three modes of interpretation.

1. *Empathic interpretation.*—An empathic understanding is perhaps always involved in the rapid comprehension of language. Bühler has insisted upon its importance in the interpretation of the introspections of trained observers, and it can not be denied that an experimenter can the better understand the language of an observer the better he is acquainted with him. Empathy is most important in the interpretation of the information of untrained observers. To just what extent an empathic translation of information into psychological terminology is reliable, it is not easy to say. Within certain limits, however, it seems safe to place dependence upon it. In psychology we find some grossly descriptive terms, which have been introduced into the science in order to classify certain sorts of very common experiences, and other more refined terms, which have come into use only as the result of the analyses of the laboratory. "Kinesthesis," for example, is a general term for "feelings of movement," a class of experiences separately distinguished by all individuals; the division of kinesthesis into muscular, articular, and tendinous sensations is a purely introspective difference that is not made in everyday life. There is little doubt that the experimenter may safely restate, empathically, in gross descriptive terms, what comes to him as information; for, if the observer reports, "I felt my hand moving," it is obvious that he means "arm or hand kinesthesis," or if he reports, "I see it plainly before me in my mind," it is obvious that he means "visual imagery;" but it is not possible, with safety, to press the classification to the application of those terms which are descriptive of experiences not ordinarily recognized as distinct. One can not say precisely what sensations composed the "arm kinesthesis," nor record the tint and chroma of the visual imagery. Empathic interpretation is, therefore, trustworthy

only when it substitutes for implications of consciousness such gross descriptive terms as were introduced by psychologists in order to designate the kind of consciousness implied.

2. *Interpretation by reference to a standard consciousness.*—When the typical consciousness for certain conditions is known, it may be used as a standard consciousness, in terms of which information, given under the same conditions, may be interpreted. Factors appearing in the same general manner both in the informational statements and in this standard average of introspections may with reasonable assurance be identified. It might appear, at first, as if the mere identification of certain factors of the informational statements with the corresponding factors of the standard consciousness could result in no additional knowledge beyond that already contained in the standard,—the method being thus reliable, but useless. Such a conclusion, however, is not correct; for, once the implication of the verbal expression is established by reference to the standard, the variations from the standard of the individual consciousnesses implied by the information can be roughly determined. If, for example, in the course of a consciousness in which strong visceral sensations ordinarily appear, the observer reports, "I feel sort of stretched inside," the psychologist is justified in interpreting this same phrase in other connections to signify visceral sensations.

3. *Interpretations of a standardized terminology.*—If the observer always reports in the terms of a standard psychological vocabulary, the danger of misinterpretation is reduced to a minimum, provided always that the terms are rigidly defined and that the observer is trained to understand the meaning of his terms as thoroughly as do his interpreters. Even under these conditions, however, an accurate conclusion is not always as easy as it appears. What we actually mean in psychology by the definition of a descriptive term is that we select some definite set of conditions and state that the experience or a certain part of the experience occurring under those conditions shall be characterized by the term in question. The selection may or may not be conscious; the conditions may or may not be experimental. We may say that we shall call the "sensation of cold" the characteristic experience that we have upon first touching ice, and we shall have adequately defined the term. If we say, on the other hand, that the feeling which we get from our joints shall be called "articular pressure," we have not sufficiently defined the quality, for we refer various sensations to the joints. We may, however, in this case state

the conditions of Goldscheider's experiment with the string and weight, and thus rigidly fix the term. The experience with the ice is common enough; that with the string and weight ordinarily needs to be reproduced in the laboratory. Both experiences may, at first, be consciously referred to as standards; the reference, however, soon becomes unconscious. It is exactly in terms of these simple standard experiences that the trained observer actually reports. His criteria of judgment may have been established in childhood, in more recent casual experience, in a psychological laboratory course, or later in the laboratory; but, unless the criteria are known, the interpretation of his reports is liable to error. Too often the supposedly trained observer remains untrained in the meaning of some particular term which he uses. We say that he "lacks introspective familiarity" with the particular processes concerned; yet it is not that the processes are unfamiliar, but rather that he has not been furnished with the criteria necessary for a classification of these processes in accordance with a psychological terminology.

On the one hand, then, the trained observer is liable to be misinterpreted in some point for which his criterion of classification is not the same as that of his interpreter; on the other hand, however, the observer, who is in general untrained, may easily be trained to report upon such distinctions if he is familiarized with standard experiences in terms of which he is later to report. Many a trained observer, so called, fails to distinguish between the experiences of warmth and heat; yet an observer, otherwise untrained, if given the two experiences with the names attached, readily learns to make the distinction. It is not safe, then, in interpreting a report to assume that, because most of the terms are unequivocal, therefore all must be; or that, because most of the statements are informational, therefore no real introspection can be expected.

The normal procedure in obtaining a description of a given consciousness may be taken to be that which we have just indicated: the observer is furnished with terminological criteria; he is then made clearly conscious of the experience to be described; he then describes the experience by reference (conscious or unconscious) to the criteria furnished. The order may, however, be reversed. After the observer has experienced and reported by reference to whatever criteria he is accustomed to use, he may then be furnished with other criteria and may be asked to state whether or not it was upon such bases that he made his judgments. This procedure is known as the method of *confrontation,* and consists in nothing more than an effort to determine, by questioning the observer after a report is made, just what was meant by the terms of the report.

II. Accuracy of Report

The report of an observer, in the meaning intended by him, may still not accurately represent the consciousness described. Inaccuracies in the account may be either those of omission or those of commission; that is to say, the observer may omit in his report factors that were actually present in the consciousness described; or he may include factors that were not present; or he may combine the two in incorrectly describing a complex factor. Inaccuracies of either class may arise from intentional misrepresentation, unintentional error [suggestibility], or from the making of irrelevant statements for relevant [scatterbrainedness, retardation, and perseveration].

1. *Intentional misrepresentation* may occur with unwilling subjects, with some insane, and, less frequently, with any observer. Simulation is common in some insanities, especially in hysteria. The Ganser symptom (if the incorrect statements which constitute it may be regarded as sometimes intentional) would also furnish instances under this rubric. Intentional misrepresentation is likely to occur with any subject under emotional conditions, as, for example, when the pride of the observer is involved. The novice in the laboratory may misrepresent when he feels that his report may reflect his ignorance, although such misstatements are generally unintentional. The usual critical methods may be employed as safeguards against intentional misrepresentation. Four criteria, which are applicable, follow.

a. Knowledge of the *general tendencies* of the observer.

b. Internal consistency of the report. Misstatement may betray itself as well in the protocol as on the witness stand.

c. Consistency of the description with regard to an average description of consciousness under the same conditions.[8] If the account of an unknown observer does not tally with the accounts of trained observers or of a large number of other observers who are consistent with one another, the reliability of his report may be questioned.

d. Consistency with regard to other facts of psychology. In psychology, as in any other science, reported facts that accord with the established facts of the science are more readily

[8] This criterion should be distinguished from the criterion for the interpretation of a report by reference to a standard consciousness [I, 2, above]. Here, if the reported pattern of the consciousness in question does not correspond with the pattern of the standard consciousness, the reliability of the report is questioned. In the previous case, the reported pattern corresponded to the pattern of the standard, and an unknown factor was interpreted by reference to the corresponding factor in the standard.

accepted than facts in disagreement with the general body of knowledge.

2. *Irrelevant statements* are often included in descriptive reports by some insane. There may be a general retardation of comprehension, so that a patient may not report upon one experience until he has had a second; or the patient may be unable to keep his attention fixed for a long time upon the topic of the report and thus, with a shift of attention, readily changes to an irrelevant topic. The extreme cases are nonsensical enough to be easily detected; but in other cases the irrelevant material is scarcely in itself distinguishable from the real description, by the side of which it may occur. Cases of material relevant to preceding consciousnesses, but not relevant to the immediate consciousness described, are common in reports from normal as well as from abnormal observers. There is a 'perseverative' tendency to report each consciousness in a series as of the same form as the last consciousness, so that the report of a change may lag behind the actual occurrence of the change. Remnants of preceding description may thus last over into subsequent descriptions after they have become irrelevant. Other cases of irrelevancy may arise where, for one reason or another, there has not been an acceptance of the *Aufgabe* to report. The degree of irrelevancy may be indicated by the four criteria already mentioned for intentional misrepresentation [*a, b, c, d*] and by another in addition [*e*].

e. Alteration of report to correspond with an alteration of experimental conditions or of questioning forms a very convenient check when irrelevant statements are suspected. The change in experimental conditions or in questioning amounts merely to the insertion of control experiments or of control questions. In extreme instances of perseveration or of retardation in the insane the meaning of the questions may be entirely altered, while the subject does not materially change the form of his reply.

3. *Unintentional error* occurs principally as the result of suggestion. In some cases of insanity, however, it may take the form of the unintentional misstatements of Ganser's symptom. Unintentional simulation is frequent enough to be regarded as a symptom of psychopathic inferiority. In both normal and abnormal cases, the error may be the direct result of some factor inherent in the mechanism of introspection, that is to say, in the way in which the descriptive report comes to be given. All the checks mentioned above [*a, b, c, d, e*] apply in this case as well as some others [*f, g*].

f. Spontaneity of report is regarded as one of the most important assurances that there is little inaccuracy due to suggestion. Reliability of report is indicated by spontaneity (1) when the information in the report is volunteered, (2) when the report is, at times, in the form of an answer that negates the implication of a question, or (3) when the verbal expression is not stereotyped.

g. Introspective report on the mechanism of description,— or at least an indication of the mechanism. If a description of the reporting consciousness is available, it will be possible to state whether the description reported attaches immediately to the processes described, or whether it attaches to representations of the processes, or whether, perhaps, it occurs automatically. When the description attaches to representative processes, inaccuracy may result from the failure of the surrogates to correspond exactly with the consciousness for which they stand. In the representation, some factors may be filled out, others may be but partially represented, others not represented at all. It may be that some of these incompletenesses and over-completenesses are intrinsic to the substitution of processes of one kind for those of another. Of the reliability of a report automatically given, even after a length of time, of the reliability of subjective certainty on the part of the observer in reporting, and of many other factors which enter into the process of description and affect the accuracy of the report, there is as yet very little known. An urgent need exists for experimental work in the investigation of the mechanism of the reporting consciousness.[9]

Experimental Series

I. Series with Cases of Dementia Precox

The reports studied are those given by eight subjects, diagnosed as cases of dementia precox, in a series of learning experiments, carried out with two pencil-mazes [M and N]. The work was performed in the psychological laboratory of the *Government Hospital for the Insane* in the summer of 1912. It is described and the reports of the patients are fully given elsewhere.[10]

II. Series with Trained Adults

It is obvious from the previous discussion that the critical examination of the introspections of abnormal subjects requires the establishment of a set of norms with which they

[9] A study of this problem is at present projected in the Cornell Laboratory.

[10] *Op. cit.*

may be compared. For this purpose, three observers, trained in introspection, were required to learn maze N, under the same conditions (as nearly as they could be duplicated) as the cases of dementia precox. The experiments were performed in the Cornell Laboratory in the fall of 1912 with the maze used for the patients. The observers were Dr. Day (*D*), graduate in psychology, Mr. Ruckmich (*R*), instructor in psychology, and Mr. Foster (*F*), research assistant in psychology. *D* and *F* were especially well trained in introspection. The observers were instructed to report "all processes relevant to the selection of the route." As a matter of fact they reported also upon other matter relevant to the total situation. It is not possible, in this article, to give the introspections even in partial detail. Summaries of the reports follow.

Observer D. 15 trials on 2 successive days. Maximal av. time, 1st day, 39.0 secs. Minimal av. time, 2nd day, 10.8 secs. Maximal single trial, exclusive of first trial, 1st day, 43 secs. Minimal single trial, 2nd day, 7 secs.

At the very beginning of learning the only prominent factors in consciousness were visual imagery and eye-kinesthesis, the visual imagery being of parts of the path, sometimes referring to the immediate position of *D's* hand and sometimes being anticipatory. She oriented herself with reference to the center or to the edge of the maze by visual imagery. Reference to the outside was also carried for her by kinesthesis in the two arms (the observers were allowed to place the left hand at the outer edge of the maze while running the course with the right hand).

As early as the third trial, kinesthesis began to become more prominent and to displace visual imagery. Arm-kinesthesis, sometimes meaning the position of the hand and sometimes meaning the direction of the course, became frequent. Later, visual imagery and eye-kinesthesis occurred only at difficult parts, often furnishing then the cue to the course. At the end of the first day, *D* reported that the center was traversed entirely in kinesthetic terms, and that beyond the center the course ran smoothly, a visual image carrying the familiarity, until interrupted by a difficulty. After that the course was taken up by "an attitudinal cue, involving both kinesthesis and visual imagery," and continued smoothly to the end, with unclear kinesthetic cues only.

On the second day, visual imagery and eye-kinesthesis soon disappeared, the directive cues remaining in terms of arm-kinesthesis. Later the kinesthesis began to become "fused together into long swings," and continued until the whole consciousness had become a single kinesthetic "swing," with fleeting attitudes occurring during its course. Organic sensations of respiration carried the *Aufgabe* to get out quickly. *D* thought that this stage was immediately preliminary to automatism.

Observer F. 38 trials on 4 days. Maximal av. time, 1st day, 204.6 secs. Minimal av. time, 4th day, 5.1 secs. Maximal single trial, 2nd day, 465 secs. Minimal single trial, 4th day, 4 secs.

At first *F* found very few "directive factors," most of the turns being made "without anticipatory imagery" and visual images having, "for the most part, only a general directive influence." Consciousness was principally composed of visual imagery and of kinesthetic and tactual sensations, referring to parts of the maze. Later the visual imagery occurred only at difficult points, notably at one point where the same mistake was made repeatedly; the kinesthetic sensations were more prominent, and the kinesthetic images acted as directive factors, that is to say, they were anticipatory to parts of the course. Visual imagery was also sometimes anticipatory.

By the end of the second day parts of the maze had become fused into "unitary, anticipatory, visual-kinesthetic complexes," with a meaning that *F* can indicate only by drawing a crooked line that traces approximately a bit of the course. On the third day, however, he reported, for the first time, that the whole course was run "within one conscious present." In it there was nothing but a fusion of kinesthetic and tactual sensations from arms, finger, and neck, with strains from the abdomen, which carried the *Aufgabe* (cf. *D*'s organic sensations of respiration). On the fourth day, the experience took definitely the attitudinal form. It was an unitary fusion of kinesthetic and tactual sensations, meaning the hand and the pencil; strain images from the two arms and the hip, meaning effort to get the hands together (a position which occurs near the finish of the course); kinesthesis from eye-movement, meaning the position of the hand (*F* thought that his eyes followed his hand); and a background of strains from the abdomen and chest, carrying the *Aufgabe* to hurry through. Visual imagery sometimes occurred with this attitude, but seems to have had very little directive significance, and was more often lacking or very scant.

At this stage *F* found the foreperiod important. In it he lived through in hasty imaginal form the whole experience of the course in more detail than he actually experienced it later. He described it as "the whole maze in a nutshell." It constituted a preparatory set for him, and, when its course was interfered with, the smooth attitude of the subsequent period was broken up.

Observer R. 38 trials on 4 days. Maximal av. time, 1st day, 35.9 secs. Minimal av. time, 4th day, 6.0 secs. Maximal single trial, exclusive of first trial, 1st day, 51 secs. Minimal single trial, 4th day, 4 secs.

The reports of *R* are more meager than those of *D* and *F* and it is more difficult to generalize from them. On the first day, there were no processes reported relevant to guidance. The procedure seemed to be largely one of trial and error. On the second day, kinesthesis was once reported as directive, and, on the third day, kinesthesis and visual imagery were each reported once as directive. A great deal of visual imagery and kinesthetic sensation occurred regularly throughout the course, but *R* could not say whether they helped him or confused him in the running. The kinesthesis often meant, not guidance, but perplexity, and was sometimes reported as meaningless. Once auditory sensations from the movement of the pencil were reported as meaning the situation and also as anticipatory to the end.

On the final day *R* continued to report visual imagery and kinesthesis, irrelevant to guidance within the maze. He stated that he found no cues at all, that consciousness seemed "to run on a lower level of attention," and that there was a "feeling," which he could not de-

scribe, that he was "perfectly independent of the movement," that he could "perform a complicated problem while running the course." From this state of affairs, he inferred that the movement had become automatic.

The reports of *D* and *F* are remarkably alike. Consciousness at the beginning of the series is very complex and broken up, visual imagery and kinesthesis being prominent. As the maze is learned, these processes assume an anticipatory function, apparently determining the course. The visual imagery gives place to kinesthesis, but recurs frequently in the most difficult portions of the route. The kinesthesis fuses together, at first for small unitary parts of the course, and later for the whole course, forming in the final stage a unitary complex, of attitudinal form, of which the core is kinesthesis, but in which visual imagery may still appear. The report of *R* is very similar as regards conscious content, although it differs in the meaning ascribed to the processes. His account of the "automatic" consciousness accords fairly well with the description of the "attitudinal" complex by *D* and *F*.

The accounts by the three observers agree with the introspections of the observers in the large "human maze," used for the study of consciousness in maze-learning by Day and the writer.[11] Here visual and verbal processes were replaced by kinesthetic, meaning the direction of turning, and the kinesthesis later lapsed into automatism.[12] The "automatism" is like that "inferred" by *R*, that is to say, it is a unitary consciousness at a low level of attention in which there is not a separate reference to the individual parts of the maze. The experience was described by one observer in the large maze as a "careening through," a phrase similar to *D*'s and *F*'s "kinesthetic swings." The support of the special introspections by those obtained with the large maze[13] makes perfectly safe the acceptance of the average account, based on the former, as a standard of comparison for the accounts of the subjects with dementia precox.

III. Series with Untrained Boys

In the study of the introspections of a class of insane patients it is natural to ask to what extent the character of the reports is due to the abnormality of the subjects and to what extent it is due to their lack of training. The question can be answered, if we establish standards of comparison with normal subjects of the same degree of training as the patients. The cases of dementia precox considered in this article were all

[11] This work is as yet unpublished. A preliminary account appears under *The Use of the Maze in Comparative Psychology, Psych. Bull.*, 9, 1912, 60.

[12] It may be thought that Day's familiarity with the typical maze-consciousness influenced her report in the learning of the pencil-maze. That this was not the case is indicated by the fact that, in her introspections on the pencil-maze, she does not report "automatism," as might have been expected, but describes a conscious attitude as a state new to her in this experience.

[13] In this work there were fourteen observers, some of whom were especially well trained in introspection. The latter included Professor Bentley and Drs. Geissler and Jacobson.

uneducated men with very poor vocabularies (with the possible exception of G). Naturally they knew nothing whatever of a psychological terminology. It was thought that their general education and command of words were about those of a twelve year old boy. Accordingly, for purposes of comparison, two boys were employed in the Cornell Laboratory to learn the maze and to give reports upon the experience of learning. One boy, *X*, was eleven years old, very alert and active, and easily interested in the work. The casual observer would characterize him as "bright." The other boy, *Y*, was thirteen years of age, slow in both movement and comprehension, and apparently less intelligent than *X*. The experimental series were conducted exactly as for the insane patients, and the questioning was of the same nature. As space does not permit a detailed account, we content ourselves with summaries of the reports.

Observer X. 98 trials on 10 successive days. Maximal av. time, 1st day, 167.9 secs. Minimal av. time, 5th day, 5.0 secs. Average time on last day, 6.4 secs. Maximal single trial, exclusive of first trial, 2nd day, 56 secs. Minimal single trial, 5th, 6th, and 10th days, 3 secs.

In the first three days, *X* frequently reported "seeing the maze in my mind," and later he said that it was as if it were "painted" or "carved" in his mind. He insisted that it was "seeing it in my mind" that enabled him to find the way, although it was suggested that other things might help. He also reported, "I have a feeling in my pencil the way it goes," but insisted that this feeling did not help him. We may presume, then, that he experienced kinesthetic sensations, meaning the maze, or the situation, or the position of his hand, and visual imagery, meaning the way to get out.

On the fourth day he reported: "I just feel the motion of my hand.....My pencil runs like a street-car on a track;" "my hand goes right through as if there were little wheels on it;" but he also said: "It just seems as if somebody were holding that same maze before my eyes, as if I were doing it without a curtain. Funny." He appeared to have set himself the *Aufgabe* to visualize; he was very eager to reduce his time and constantly wondered "how it looked." When asked later, however, if he could "do it without seeing it," he replied, "I'm quite sure I could. I guess I could get the motion of my hand as easy as I could see it in my head." His procedure on making a mistake suggested that kinesthesis was the guiding cue, for he would pause after a rapid correct movement, move back and forth hesitatingly, and then finally shoot off rapidly in the right direction, very much as the pianist, when playing from memory, stumbles through a difficult passage and then catches the swing of the movement again.

On the seventh day, he found that he could no longer increase his speed, and consequently lost his keen interest in the problem. With interest the *Aufgabe* to visualize seemed to lapse, for only very infrequently did he now report "seeing the maze." Often he insisted that he did not "see the maze," and he stated time after time, "Felt

the motion of my hand. That's all." His behavior had become typically motor, and frequently, like the piano player, when checked in a difficult place, he would get past by going back and taking a "running start," which would carry him by.

On the eighth day, he once reported that he "thought of nothing" while going through, and, on the tenth day, we find this condition frequent. Only when he "got caught" did he "feel the motion of his hand." Apparently this stage corresponds to the "automatic" or "attitudinal" consciousness, reported by the trained observers.

Observer Y. 100 trials on 9 successive days. Maximal av. time, 1st day, 41.7 secs. Minimal av. time, 8th day, 7.0 secs. Av. time on last day, 7.6 secs. Maximal single trial, 2nd day, 109 secs. Minimal single trial, 8th day, 6 secs.

At first *Y* was unable to comprehend the meaning of the questions asked. His reports suggested verbal imagery, but were very indefinite. On the second day, however, he noted an orienting cue in the fact that his hand "felt different" when it was near the edge. Later he said, "When my two hands get near together, I can kind of feel it in them,"—the kinesthetic cue mentioned by *D* and *F*.

At the end of the third day, *Y* suddenly reduced his time in a trial in which he changed his whole bodily attitude, so that, instead of gazing off vacantly to one side, he fixated the curtain steadily. He reported: "Usually I just kind of look off and go through; but this time I looked hard at the curtain and tried right hard." Later he said: "There is a kind of picture of the maze,—part of it. I can't tell all the parts of it;" "I can't see the color of the board and all that. I can just kind of see the path part of it." Thus the presence of visual imagery seems assured.

Of the parts for which he reports no visual imagery, he said: "I can just tell by the way my hand goes;" "one place I can't see the maze, but I know from the way my hand goes that I'm getting near the place where I get out." The form of expression in this case, as well as in the others quoted, was unsuggested to *Y*, although resembling closely the words of *X* and of the dements.

On the last three days, *Y* continued to indicate both visual imagery and kinesthesis, although he is given during this time 42 of the 100 trials, in order to allow the habit to become entirely motor, if possible. He evidently found the work monotonous, for his answers appeared stupid and his phraseology stereotyped and parrot-like. He worked uninterestedly, gazing off to one side. He was unable to draw the route, although he reported that he "could see it in his mind." His indefinite fixation and his inability to draw suggest that visual imagery was perhaps less and kinesthesis more important than his indefinite reports would indicate.

Critique of the Reports

Let us now proceed to a critical examination of the introspective reports made by the eight cases of dementia precox. As we cannot reprint them here, we shall refer to them as explicitly as possible.

It would not be fair to seek for evidence of reliability or of unreliability in the reports of all the subjects taken in a mass. Altogether enough positive or negative instances might

be found to establish their reliability or to overthrow it, while the proof might not be at all conclusive in any one of the individual cases. We must begin by measuring the report of each subject by reference to the criteria we have already established, and then see what our net result has been. We shall make the separate surveys as brief as possible, referring in each case to five factors of reliability,—a modification of our original schema, which we shall discuss later.

Subject A.

Intelligibility. A uses such phrases as "I can tell by the touch of the pencil," or "I get out by just following the pencil" for kinethesis, if the empathic interpretation be accepted. "I just came to have the right feeling for the passage; I can't explain it," coming at the end of a series, may be taken to stand for the conscious (kinesthetic) attitude reported by the normal observers.

Misrepresentation. No tendency toward dishonesty was noted. The internal consistency is good, there being only one contradiction of statement, and that one influenced by suggestion. The report—as much of it as is interpretable—is consistent with the average consciousness.

Irrelevancy. No tendency to talk "off the point" was ever observed. The report is consistent. For these reasons there was no attempt to vary conditions in order to note the effect on the report.

Suggestibility. A was ordinarily very suggestible. He showed a tendency to answer all possible questions by "Yes, sir" or "No, sir," according to their implication. He did not "resist suggestion" in the Binet test with lines. On the other hand, when the questions required considerable thought before answering, he would as frequently negate the implication of the question as agree with it. Once, in answer to suggestive questions, he apparently contradicted himself. At all other times the report is consistent with itself and with the average course of consciousness. In the latter connection, the indefinite verbal expressions coming at a time when the attitudinal consciousness might be expected, are worthy of note. A here reports: "I just came to have the right feeling for the passage;" "I just kept feeling for the doors, but I can't explain how it was;" "I don't know how to explain it. It was just the feeling for the gates and thinking how it must be,—from practice." The phrases already noted illustrate the spontaneity of expression. None of these phrases, apparently, was suggested.

Completeness. A was not at all communicative and had to be constantly urged to reply to questions fully. There is no internal evidence that his reports were not complete, but comparison with the detailed protocols of the trained observers shows them to be very meager. A does not indicate spontaneously that he is giving an account of the entire experience.

Subject B.

Intelligibility. B used the terms "general opinion" and "general imagination" very indefinitely, but later identified them as "like look-

ing at a picture in the dark, when you can about half see it" and as "something like looking at a map." Later he denied having a "general opinion of it," and stated that he "just knew how to go," that he "had it in his mind," and that "somehow when I get in the passages it just seems as if I knew how to go." In terms of the average consciousness, these expressions must indicate kinesthesis, the last, perhaps, the semi-automatic movement.

Misrepresentation. B was inclined not to talk much, except about his troubles, and sometimes resented being plied with questions. Such answers as he gave, however, appeared to be sincere. He was not inconsistent with himself, and his sudden change of report from "general opinion" to "just knowing how to go" corresponds well with the change from visual to kinesthetic cues in the average consciousness.

Irrelevancy. While B frequently broke into his report to talk about his troubles, the line of demarcation between the two topics was always too distinct to permit the confusion of material. His report is internally consistent. A change of report, interpreted as a change from visual to kinesthetic cues, occurs in connection with the change in behavior to the typically kinesthetic form, indicating that the old form of expression did not last over after the experience it described had ceased,—that is to say, B did not make the "perseverative" error.

Suggestibility. B would stick to a point in spite of mild suggestive questions to the contrary. His report is consistent with itself and with the average consciousness. His expressions, as a rule, are not original, although they sometimes pass beyond triteness. Note, for example, the phrases quoted above for visual imagery.

Completeness. B was verbose about his troubles only. His replies to questions were very meager. Compared with the average account, he indicated only the three main phases of the course of consciousness.

Subject C.

Intelligibility. With maze M, C reports doubtfully, "I sort of see what it looks like," adding at one time, "Of course, I don't see it; I imagine it." He is, however, uncertain, and his report is not very intelligible. With maze N, however, he notes, "Seems like you just keep your pencil going like," and later, "I just sort of feel with my hand where the next door is going to be,"—phrases which one may take to mean kinesthesis. Such an interpretation is justified by his later usage: "Only when my hand stops going, then I think of it [the maze]. It doesn't pay to stop.....The stopping mixes me up;" "sometimes, when I go wrong, I think more about it;" "I don't think about anything" (while going through),—a positive assertion. These three expressions evidently refer to the smooth course of the habituated kinesthetic consciousness, the heightening of consciousness when the course is interrupted, and the final, low-level, attitudinal stage, respectively.

Misrepresentation. C was very sensitive and confiding, and appeared to try genuinely hard to do everything that he was asked to do. There were no gross inconsistencies in his reports, and his accounts, as indicated above, tallied in many respects with the average account.

Irrelevancy. C always made it possible to distinguish between relevant and irrelevant remarks, although he showed a tendency to introduce the latter. When the form of questioning was changed, he al-

ways responded to the altered meaning. His reports were, on the whole, consistent.

Suggestibility. He showed a tendency to agree with the experimenter in conversation, although this tendency did not appear very prominently in the reports. Once he contradicted himself, apparently as the result of suggestive questioning, and then, observing his mistake, modified it to make it partially consistent. In the first paragraph above, we have already partially noted to what extent his account tallies with the normal. There should also be mentioned his observation of the fact that the first part of the course was oriented with reference to the center,—a fact brought out by the normal observers. The quotations already given show a fair degree of spontaneity of expression.

Completeness. C, although bashful, talked freely when his confidence was gained, and gave thus a much fuller report than the two subjects already considered. He noted, as has been seen, a fair number of points observed by the trained observers.

Subject D.

Intelligibility. D was very indefinite. It was not easy to say what he meant by "knowing how to go" and by "putting his mind on it," or whether he had any visual imagery. His "kind of indefinite feeling" of how to go may be taken to be kinesthesis,— emphatically interpreted; and his later report, from comparison with the reports of the trained observers, is certainly kinesthesis: "That maze, there's a kind of give to it,—a kind of give where the openings are. I remember the way it gives, the way it feels when you push against the openings." Once he states that he got out "by the sound of the pencil.... There was a sort of indefinite rhythm about it. It played a tune,"—a description much like that of *R.*

Misrepresentation. D showed no tendency toward intentional misrepresentation, his inconsistencies being more readily explained upon another basis.

Irrelevancy. He exhibited a very baffling tendency to introduce irrelevant material into his answers without any warning. Many parts of his reports were discarded, because it was not possible to determine whether he was answering the questions or talking nonsense. The following excerpt from the notes of the experiment will illustrate the point: "Q. How did you remember how to get out? A. By the feel. Q. What do you mean by the feel? A. The way my hands feel and the way my eyes feel. Q. Do you often have a feeling in your eyes? A. Yes, they go round and round, like snakes. Q. How do you mean? A. Like snakes in a tobacco bag. I think there's gun-powder in them." And so on. At first it appears as if D were reporting a kinesthetic cue. Next one suspects eye-kinesthesis. And then it developes that he is talking without reference to the questions, although it is not possible to say just when the nonsense began and the sense ended. This tendency was very disconcerting, and makes one always uncertain of the value of his reports. It was necessary constantly to change the sense of the questions, in order to see if his answer changed accordingly. He did not, however, fall into this trap very frequently. The full record of D's work (not the published summary) contains many inconsistencies, quite evidently due to his persistence in bringing in irrelevant material.

Suggestibility. D was slightly negativistic, but the trait was not evident in his answers. There was no evidence of positive suggestibility. As much of his report as is safely interpretable is consistent, and accords with the standard account. He shows very little originality of expression except in his account of the "give" to the maze, quoted above.

Completeness. D was frequently subject to spells of mutism, and, except on one or two occasions, never talked freely. His reports, compared with the standard, must be judged very incomplete.

Subject E.

Intelligibility. E was able to explain his terms fairly explicitly. He distinguished between "memory," which is a visual image of the whole maze ("the looks of the maze with the wrong ways to go in it too"), "forming it in the mind," which is the visual imagery of the part immediately concerned ("the way it looks to go right;" without "the wrong ways to go in it"), and the "feel," which is kinesthesis. He described the "feel" and the "looks" quite thoroughly, so that there is little doubt of the empathic interpretation. This fact is particularly striking, because his consciousness did not follow the average course, but remained persistently visual. If E's account had not been especially clear, it would not have been possible to determine this fact with certainty.

Misrepresentation. E coöperated heartily in the work throughout and gave every evidence of sincerity. His report is full and internally consistent, although it departs to some extent from the average course of consciousness.

Irrelevancy. E's answers were always pertinent. He showed no tendency to talk nonsense. At one time, he reported, first, visual cues, then a conscious decision to rely upon motor guides, and finally a return to visual guidance, in successive reports, so close together, that there would seem to be little 'perseverative' tendency for the expression in one report to lag over into the next.

Suggestibility. No tendency toward suggestibility was manifest. The reports were internally consistent. The inconsistency with the average consciousness has already been mentioned. E's descriptions are, however, so explicit, for an untrained subject, that they can be taken as forming a clear exception to the normal course. Moreover, it is not to be said that E would not have formed a purely kinesthetic habit had the series been prolonged. His persistent visual imagery, at any rate, may have been due to his keen interest in the task and his effort to reduce his time,—an occurrence that happened in the case of observer *X* (*cf.* p. 158). The spontaneity of his replies is especially marked. Many of them, including those quoted in the first paragraph above, were volunteered, and not given in response to questioning. Besides the points noted, he reports, without suggestive questioning, kinesthetic cues for limiting arm movement ("I felt I was moving my hand too far"), anticipatory kinesthesis on passing through a door, and an anticipation of the movement in the foreperiod ("you have to get the movements formed in your mind before you start out;" *cf.* the foreperiod of the normal observer, *F*, "the whole maze in a nutshell," p. 156).

Completeness. E was free and very talkative. The explicitness of his reports has already been indicated.

Subject F.

Intelligibility. F spoke of "feeling the way" and later, when he was lost, of "going back for a fresh start." His procedure was typically motor, that is to say, he got rapidly over a part of the course, stumbled about, picked up his cue, and then went rapidly over the next part. Sometimes he would go back a little way and get a running start, which would take him past the difficult place. As this performance is a condition for kinesthesis. we may interpret his expression accordingly. There was no hint of visual imagery.

Misrepresentation. F ordinarily seemed to be truthful and sincere. His report is scarcely explicit enough to be internally inconsistent. It does indicate the form of consciousness that would be expected from the behavior.

Irrelevancy. F was, in general, careless, both in manner and in form of expression. He showed a slight tendency to answer questions irrelevantly, but, although he shifted easily to disconnected topics, there was generally a change in attitude, that protected his interpreter from error. When the form of the questions was changed, he was always sensible to the difference.

Suggestibility. He did not appear especially suggestible in ordinary conversation. He did not, on the other hand, exhibit very much originality of expression. He did observe the necessity of going rapidly and not "thinking much" about the task, facts which support his statement that he "goes by the feel;" and at the end he noted that "it takes just one whole draw" to get out,—the kinesthetic attitude of the normal observers *D* and *F*.

Completeness. F talked incessantly, but did not keep his attention readily upon the maze problem. His lack of interest in the introspective side of the problem partially accounts for the lack of detail in the report.

Subject G.

Intelligibility. G insisted: "I see it in my mind,"—visual imagery. His references were made explicit by describing the turns "seen," as a fire-place with a mantel, as a street, as a casaban effect on a curtain, and so forth. Later he reported being guided in part by the "sense of feel" or "touch," which he opposed to the "sense of thought," the latter possibly meaning visual imagery or perhaps merely a heightened consciousness. He said: "It brings the two senses together, the sense of feel and the sense of thought.....There are a number of places where I'm uncertain; then the thought comes in. The sense of feel is when I'm all right." In terms of the average consciousness, then, the "sense of thought" is a heightened consciousness and probably a visual one.

Misrepresentation. G was seemingly perfectly honest, and his reports are consistent with themselves and with the average course throughout.

Irrelevancy. Very occasionally G may be suspected of having introduced irrelevant material into his answers, but the proof is not positive. He always replied pertinently to questions. His reports were uniformly consistent.

Suggestibility. G was not evidently suggestible. His reports showed unusual originality of expression. The comparisons quoted in the paragraph above and many others of like nature were made spontaneously.

Completeness. G was a fluent and persistent talker. If he could have devoted all his remarks to a description of conscious processes his reports would have been full indeed. As it is, they give only the general outline of the conscious course.

Subject H. (Series cut short; less practice than other subjects.)

Intelligibility. H reports, "I follow the pencil," "I go with the pencil," "I just feel my hand go through,"—expressions which may be emphatically interpreted as kinesthesis. Nothing else in the protocols is positive. The indefinite indications of visual imagery are possibly the result of suggestive questioning.

Misrepresentation. H appeared to be honest about all matters except his delusions. His report is too meager to give opportunity for measurement on the basis of internal consistency, although there is one contradiction, which seems to be the result of suggestion. As far as it goes, it is in accord with the normal course.

Irrelevancy. There is no especial reason to suspect irrelevant material in the reports.

Suggestibility. H appeared in ordinary conversation to be very suggestible, that is to say, he was very sensitive to the meaning of everything that was said to him. Once, when asked if he "saw the maze in imagination," he replied that he did, but his later reports throw doubt on the presence of visual imagery. Still the proof is not conclusive. At no other time does he contradict himself. His expression shows no more originality than is indicated by the quotations already made.

Completeness. He was taciturn about everything except his troubles, and toward the end of the series resented being asked too many questions. At best he indicated only the general character of his consciousness.

It is now our task to bring together the loose ends of our criticism, to generalize so far as generalization is permissible. We can best do this by means of a table, in which rough scores show approximately the various degrees of reliability indicated by each criterion. Nothing but the crudest sort of quantification is intended, and even that represents nothing more than the personal estimate of the writer.

The captions of the table, it will be noticed, do not correspond exactly with those given in the discussion of the criteria of reliability above. The change is a simplification, resulting from the omission of the possible criteria that could not be determined, or were not applicable, in the present cases. The criterion of "consistency with regard to other facts of psychology" is omitted throughout, because it applies to generalizations of a larger scope than any undertaken in the present study. The "mechanism of description" as a criterion of reliability is not considered, because no accounts of the means of

reporting were taken with the insane patients,[14] and because too little is known of the relation of the mechanism of reporting to the accuracy of the report. With this factor omitted, the only sort of unintentional errors that concern us we may group under the general head of "suggestibility." The degree of "completeness" should properly be indicated by all the criteria listed under "intentional misrepresentation," "irrelevancy," and "unintentional error," for the errors of omission are coördinate with those of commission; but for all practical purposes in very crude reports, such as we have to consider, completeness is indicated only by the general tendency of the subject to talk freely of his experiences and by a comparison with the standard consciousness. The latter is by far the more important.

The method of scoring adopted in the table is to indicate by "1" the very greatest, and by "5" the very least, degree of reliability. The intermediate degrees are represented by "2," "3," and "4," thus throwing all measures of reliability into a rough five-fold classification. An average (in italics) is shown for each of the main factors. It is not an arithmetical mean, but an estimate based upon all the times available. It might be regarded as a weighted mean. It is obvious that one criterion, strongly indicating unreliability, may readily outweigh others, which fail to indicate unreliability, for the criterion may be selective for different types of error, and a single one may imply a high degree of unreliability of a certain kind. When a criterion is obviously to be disregarded in favor of other measures, the score is placed in parentheses. In such a case, the score bracketed is generally based on much more meager evidence than are the other scores.

We have also scored in the table, for purposes of comparison, the reports of the untrained boys and of the trained observers. Lack of space forbids a separate discussion of these reports, but a review of the summaries given and a comparison with the protocols of the patients should convince the reader that they are approximately correct.

The bold-faced type shows the averages for the different classes of observers.

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[14] It is not impossible that some indication of the reporting procedure might be got from untrained observers. An attempt to bring out information on this point with the boy *X*, indicated that his reported description in general attached immediately to imaginal revivals of the processes in the original consciousness, or at least to representative processes from the same sense-department. When asked, for example, how he was sure that he had both seen and felt his way, he replied, without further suggestion, "Because I still see it, only it's fainter; and I feel it still, only I don't feel it very much." On the other hand, *Y*, after being given a special series with a view to determining his mode of reporting, was unable to make any satisfactory replies at all. It may be that trained observers differ in the ease with which they assume the introspective attitude toward certain experiences. Both *D* and *F*, who, however, had had training along these lines before, found no difficulty in describing the reporting consciousness; on the other hand, the description was difficult for *R*, who found that the double *Aufgabe*, to report and to report on the reporting, was distracting.

INDICATED RELIABILITY OF REPORTS

Under each factor of reliability are placed the criteria which indicate reliability for that factor. Indicated reliabilities for each criterion are scored from 1 to 5, 1 being the greatest, and 5 the least, reliability. The average score for each factor is given in italics. It is not the arithmetical mean, but an estimated classification based on the relative values of the different criteria. Numbers in parentheses signify that the criterion so marked is for other reasons discounted as an indicator of the reliability of that particular factor.

Factors of Reliability	As indicated by:	Observers														
		Dementia Precox									Untrained Boys			Trained Adults			
		A	B	C	D	E	F	G	H	Av.	X	Y	Av.	D	F	R	Av.
INTELLIGIBILITY																	
	Empathic interpretation	3	4	3	5	1	3	2	3		2	3		1	1	1	
	Reference to a standard consciousness	3	2	2	5	2	3	3	4		2	3		1	1	1	
	Standardized terminology	–	–	–	–	–	–	–	–		–	–		1	1	1	
	Average intelligibility:	*3*	*3*	*3*	*5*	*2*	*3*	*2*	*3*	3.0	*2*	*3*	2.5	*1*	*1*	*1*	1.0
ACCURACY																	
	MISREPRESENTATION																
	General tendencies (truthfulness) of observer	1	1	1	1	1	1	1	1		1	1		1	1	1	
	Internal consistency of report	2	1	1	?	1	?	1	?		1	1		1	1	1	
	Consistency with regard to an average description	1	1	1	?	(3)	2	1	2		1	1		1	1	1	
	Average misrepresentation:	*1*	*1*	*1*	*1*	*1*	*1*	*1*	*1*	1.0	*1*	*1*	1.0	*1*	*1*	*1*	1.0
	IRRELEVANCY																
	General tendencies ('perseverative', retardative, *etc.*)	1	2	1	5	1	2	2	1		1	2		1	1	1	
	Internal consistency of report	1	1	1	4	1	?	1	?		1	1		1	1	1	
	Consistency with regard to an average description	1	1	1	3	(3)	2	1	2		1	1		1	1	1	
	Change of report with a change of conditions	?	1	1	4	1	1	1	1		?	1		–	–	–	
	Average relevancy:	*1*	*1*	*1*	*4*	*1*	*2*	*1*	*1*	1.5	*1*	*1*	1.0	*1*	*1*	*1*	1.0
	SUGGESTIBILITY																
	General suggestibility of observer	3	1	2	1	1	1	1	2		1	2		1	1	1	
	Internal consistency of report	2	1	2	?	1	2	1	?		1	1		1	1	1	
	Consistency with regard to an average description	1	1	1	3?	(3)	2	1	2		1	1		1	1	1	
	Change of report with a change of conditions	?	–	–	?	1	–	–	–		–	–		–	–	–	
	Spontaneity of report	3	3	3	5	1	4	1	4		3	3		1	1	1	
	Average suggestibility:	*2*	*2*	*2*	*4*	*1*	*3*	*1*	*2*	2.1	*2*	*2*	2.0	*1*	*1*	*1*	1.0
	COMPLETENESS																
	General tendencies of observer	5	5	4	5	(1)	4	(1)	4		4	5		1	1	1	
	Consistency with regard to an average description	5	5	4	5	3	4	4	5		3	4		1	1	2	
	Average completeness:	*5*	*5*	*4*	*5*	*3*	*4*	*4*	*5*	4.4	*3*	*4*	3.5	*1*	*1*	*2*	1.3

Referring to the table and recalling the citations which we have made from the protocols, we find, as we should naturally expect, that the reports of the dements suffer most in incompleteness. There is a considerable difference between subject E, who coöperates heartily and gives a clear, explicit account of all that he can recall, and subject D, whose answers are very indefinite and fragmentary; but even the report of E is much less detailed than those of the trained introspectors. The other patients, besides E, give, as a rule, only the general character of the more prominent processes. It is impossible to get from them accounts of the more subtle means of orientation, of the exact course of conscious events while the maze is being run, or even of the exact character of the experiences which they do indicate or mention. One has only to read the introspections of the trained observers to realize how schematic is the account furnished, even after much patient questioning, by these untrained subjects. On the other hand, one should never lose sight of the fact that something of attested reliability is obtained, and that, for some purposes, that something is all that is required.

Next to completeness the reports are most deficient in respect to intelligibility. It is hard to interpret them with the explicitness that is often desired. We tend empathically to interpret such phrases as " I see it in my mind " or " I just felt as if my hand were being moved " as visual imagery or kinesthesis. If the phrases are varied or supplemented by others of like nature, we become more positive. If they appear in accordance with an already established course of consciousness, we are reasonably assured of their meaning. On expressions of this sort the entire interpretation in the cases considered has had to be based. The dements had, of course, no command of a standardized terminology, and there was no time to train them in the meaning of any particular terms, although there is no apparent reason why they could not have been so trained.

The chief source of error in the interpretable items reported is that which we have called suggestibility. Three subjects showed a general tendency to reply according to the implication of the question. Three indicated suggestibility by conflicting statements in the reports. All, except E and G, more or less lacked the spontaneity of expression, which may be taken to indicate freedom from suggestion. D and F were especially poor in this respect.

There was some tendency—a tendency which might be expected in cases of dementia precox—to make statements,

while reporting, entirely irrelevant to the questions asked or to the whole situation. Frequently the statements have a bearing on the delusional troubles of the patient, but this is not always the case. The confusion results, doubtless, from the inability of the subject to keep his attention concentrated upon a single extraneous topic for any considerable period,—a deficiency characteristic of dementia precox. With some subjects, however, the shift of attention is so marked, by a change in bodily attitude, that there is no danger of confusing the pertinent with the irrelevant material. With others, who talk freely, the meaning of the remarks is sufficient to protect the experimenter from error. With a subject, however, who answers only in monosyllables or short phrases, as did subject D, the shifts of attention are not obvious, as the instance quoted above clearly shows (p. 162), and the danger of error is large. D is the only subject in whom this tendency to report irrelevant material throws serious doubt on the reliability of the protocols.

No evidence at all of intentional misrepresentation was found in the insane subjects tested. An investigator might not, of course, always be so fortunate. The patients were not always truthful when in the wards. The experimenter, however, made an especial effort to secure their confidence and to treat them sympathetically, and it may be that his attitude determined their behavior.

The writer was struck, when performing the experiments with the untrained boys, with a great similarity between the reports of the boys and the reports of the cases of dementia precox. The type of questions required, the form of the answers, the phraseology, the amount of material reported, all seemed to be very much alike in the two cases. We now see that according to our system of scoring, the indicated reliability of the records of the dements is only a little less than that of the normal boys. The patients were considerably less complete in their reports than the boys; but they were only slightly less intelligible. They were somewhat more given to irrelevant statement, but they were practically no more suggestible. All observers, normal and abnormal, were scored in the highest class for honesty. The patients, in fact, rank so little below the boys, that individual patients rank higher than the average for the boys. Both E and G average, on the whole, higher than the average for the boys, E not ranking below them in a single item. C is just equal in reliability to *Y*, the poorer of the two boys. If subject D had been excluded, the record of the patients would have approximately equalled that of the

boys in everything but completeness. D is responsible for almost all the deficiency indicated under the rubric "Irrelevancy," a deficiency which is, however, undoubtedly an immediate result of mental disorder. The differences in intelligibility and in completeness of reports between the dements and the untrained normal subjects seem to be nothing more than one which results from a poorer command of verbal expression by the patients, coupled with an inability to observe, or at least to describe, details,—a condition that we might find in any untrained observer with a poor command of language.

Conclusions

1. Persons with dementia precox can, under experimental conditions and without prolonged special training, give reports indicating the general trend of consciousness.
2. The phraseology of the reports is simple and naïve, and its interpretation depends upon an empathic understanding of certain forms of expression or upon a knowledge of the facts of the average consciousness under the conditions of the experiment.
3. The reports are very incomplete as compared with the reports of trained normal observers, but they indicate reliably the general character of the contents of consciousness, and may in some cases include implications of special and less prominent processes.
4. The dements are more suggestible than trained observers.
5. There were no indications of intentional misrepresentation in the reports of the patients experimented upon.
6. Some of the reports are rendered unreliable by the tendency of the subject to make no distinction, apparent to the experimenter, between statements that are relevant and statements that are irrelevant to the situation or to the questions asked. This confusion may arise from the state of constantly shifting attention, characteristic of dementia precox, which arises when a subject is required to attend to a situation foreign to his usual course of ideas.
7. There is great individual variation between the dements in the factors indicating the reliability of the reports.
8. The reports are, on the whole, of about that degree of reliability that is found in reports made by untrained observers with little education and a poor command of language, and appear to differ from these reports in no characteristic way other than in the introduction of irrelevant material.

EXPERIMENTS ON COLOR SATURATION[1]

By L. R. Geissler

The following experiments are a part of a more elaborate study of color saturation which has been interrupted for the present, but may be resumed later. Nevertheless, the results thus far obtained are presented in their unfinished form, since they may be of use to others interested in the problems of color-vision.

The only experimental work on color saturation which the author has found in the psychological literature dates back to the sixties of the past century. In 1865 Aubert published[2] some measurements on the liminal sensitivity to color saturation. He determined the smallest sector of color that would appear as a just noticeably colored ring on a rotating white disc and found it to be 2 to 3 degrees, while on black and gray discs even smaller sectors were recognized. He also made some experiments on the differential limen of color sensitivity; and he found that on a black background the stimulus-increment for orange must be 0.95 per cent, for blue 1.54 per cent, and for red 1.67 per cent, in order to produce a just noticeable increase in saturation. Aubert used the Masson-Maxwell disc, and observed under ordinary daylight illumination. It is not stated whether he employed any observer besides himself; but in a later summary of his results[3] he remarks that they were confirmed by J. J. Müller,[4] who does not give any account of his experiments, and by M. Woinow.[5]

Our problem was to determine whether the number and sizes of colored stimulus-increments corresponding to just noticeable saturation differences would lend themselves to a measure of saturation. We began our attack upon this problem from the two extremes,—by gradually reducing a maximally saturated pigment-color, and by introducing more

[1] From the Physical Laboratory of the National Electric Lamp Association, Cleveland, Ohio.

[2] H. Aubert, *Physiologie der Netzhaut*, Breslau, 1865, 138-150.

[3] H. Aubert, *Grundzüge der physiologischen Optik*, Leipzig, 1876, 531-532.

[4] J. J. Müller, Zur Theorie der Farben, *Graefe's Archiv f. Ophthalmologie*, xv., *Abth.* 2, 1869, 243.

[5] M. Woinow, Zur Frage über die Intensität der Farben-Empfindung, *Graefe's Archiv f. Ophthalmologie*, xvi., 1870, 251-264, esp. p. 256.

and more color into a colorless stimulus. Both groups of experiments were performed under an artificial daylight illumination of constant intensity and constant spectral composition. The rotating double color-disc was mounted on a Lummer-Brodhun color-mixer with peripheral attachment and viewed against a perfectly neutral grey background. The stimulus was exposed for three seconds at a time and the interval between two exposures was always made long enough for after-images to disappear. Our colored stimuli were Zimmermann papers of the following hues: red (d), yellow (h), green (l), and blue (n), mixed with Zimmermann greys of corresponding degrees of brightness. The colors were equated with the greys by the method of flicker-photometry, the voltage readings of a Weston electric speedometer being taken as an indirect indicator of the speed of the color-mixer. The grey which at lowest voltage, and hence at slowest speed, mixed with a color without flicker, as determined by several observers, was regarded as being of the same brightness as the color. The equations thus found agreed in the main with the introspectively estimated equations made by a number of observers well trained in photometric work, as the following Table shows, in which the small letters indicate the greys as taken from the Zimmermann set of thirty-five greys.

Equation of Color and Greys

Natural Daylight

Obs.	*Yellow*	*Green*	*Red*	*Blue*
A	a-b	e	k	r
B	b	f	m	p
C	b	d	n	q

Artificial Daylight

A	a-b	e-f	h	s
B	c	d	l	p
C	b	e	m	q

Flicker-Photometry

b	d	m	r

Since grey *a* is the lightest in the series, almost a white, the order of our colors according to brightness is from brightest to darkest: yellow, green, red, and blue. The difference between yellow and green is the smallest; that between green and red the largest; and that between red and blue is intermediate. The use of the peripheral attachment of the Lummer-Brodhun color-mixer makes it possible to compare a colored

ring on the outside, which is kept constant, with a colored disc on the inside, which can be varied during rotation.

In our first group of experiments we worked with red only. Two of the four observers were more or less trained in psychological experiments, namely, Miss Wilma Ball (B), of Cleveland, who kindly volunteered her services in the interest of psychology, and the writer (G). The other two were Messrs. G. Cadish (C) and L. Krill (K), both employed in the Laboratory and familiar with photometric work and observations.

Employing the 'method of limits' we began with maximal saturation, that is, 360 degrees of red, both outside and inside, and gradually added small amounts of grey (m) to the inside, until it looked just barely less saturated than the outside. Reversing the procedure, the inside was made definitely less saturated, and then step by step more red was added until it looked like the outside. At least six such pairs of series were taken, and in half of them the judgments 'more saturated,' 'less saturated,' 'equal,' or 'doubtful' referred to the outside as compared to the inside, while in the other half the reverse was the case. The average amount of grey thus found to be necessary in order to make the inside look just less saturated than the outside was then introduced in the outside, so that now the inside had to be changed still more, before it again seemed to be just less saturated. Then the outside was again reduced by the corresponding amount of grey; and similar series were taken to determine the next step. This procedure was continued until the outside had been reduced from 360 degrees color to about 300 degrees color *plus* about 60 degrees grey. It was intended to continue the method for red down to the color-limen, and then to repeat it with several other colors, at least at certain stages between 360 degrees and 0 degrees; but unavoidable external circumstances prevented this. It was possible to carry on the experiments for red with two observers, B and G, only in the regions of 240-222 degrees and 120-110 degrees of red *plus* the corresponding amount of grey. With C and K a few unsystematic observations were made at the same places, giving similar results.

The detailed results are presented in the following Table, in which the stimulus-increments are given for each observer. The figures in the odd columns indicate the size of the red sector in the outside ring at the stage corresponding to the preceding and succeeding steps. Observer C, for example, distinguished 360° of red in the outside from 350° red *plus*

10° grey in the inside, and the latter amount of saturation in the outside from 346.5° red *plus* 13.5° grey; or in other words, the third degree of saturation was produced by adding 3.5° of grey to the previous amount of grey. In calculating the Av. and MV. for C and K the last two approximate values were included.

B		C		G		K	
360.0°		360.0°		360.0°		360.0°	
	13.0		10.0		10.0		10.0
	9.0				6.5		8.0
	7.0	350.0°	3.5		7.5		5.0
	6.0		3.5		7.0		5.0
			5.0		5.5		4.5
325.5°	4.0		4.0	323.5°			6.5
	3.5		3.0		3.5		5.0
	4.5		4.5		3.0	316.0°	
	4.0		3.5		3.5		
	5.0		3.0		3.0		
302.0°			4.0		4.0		
			4.5		4.0		
		311.5°		302.5°			
240.0°		240.0°		240.0°		240.0°	
	3.0				3.5		
	4.5	about	4.0		5.0	about	4.5
	5.0				3.0		
	4.0				3.0		
223.5°				225.5°			
120.0°		120.0°		120.0°		120.0°	
	3.5				2.5		
	3.0	about	3.5		3.5	about	4.0
	4.0				3.5		
109.5°				110.5°			
Average and Mean Variation of last 12 steps 4.0±0.5		Average and Mean Variation of last 12 steps 3.85±0.5		Average and Mean Variation of last 13 steps 3.5±0.42		Average and Mean Variation of last 7 steps 4.9±0.51	
0.0°	1.25	0.0°	0.81	0.0°	1.56	0.0°	1.31

The Table shows at a glance that, so far as this particular pigment red is concerned, the stimulus-increments corresponding to just noticeable saturation-differences are approximately constant at such different stages as 325° red *plus* 35° grey, 230° red *plus* 130° grey, and 110° red *plus* 250° grey. It seems fair to assume that the increment-values would have remained constant at the intervening stages, and perhaps also at a stage not far removed from the absolute color-limen, which is given in the last horizontal row of the Table. If this result is verified by later experiments, it will allow us to estimate the approximate number of least perceptible differences in the saturation of our pigment-red, and to say that it is close to one hundred. We have a few systematic data for observer K on green mixed with grey *d*, which is thus a good deal lighter and, at the same time, less saturated than red. It is significant that the first four stimulus-increments required to reduce green by smallest possible changes in saturation are considerably higher than those for red, namely, 17, 14, 9, and 12, as compared with the corresponding values for red, which for K were 10, 8, 5, and 5. The difference seems to indicate that the original saturation and brightness of the color measured have some very definite influence upon the size of the just noticeable saturation-differences. If these results could be confirmed and extended to other colors, they would simplify the problem of measuring saturation far beyond our expectation; but at present we must refrain from basing any speculative conclusions upon them.

Our second group of experiments was performed under the same external conditions and by the same method. In addition to red, the colors yellow, green, and blue were used; and besides binocular vision we also made determinations for each eye separately. There were altogether nine observers: C, G, and K of the previous group, Miss M. Majerus (M), Messrs. C. F. Lorenz, Ph.D. (Lo), M. Luckiesh (Lu), A. G. Worthing, Ph.D. (W), of the Physical Laboratory, and Mrs. L. R. Geissler (SG) and Mr. F. Aeberli, M. D. (A), of Cleveland, O.[6] The observers thus represent various degrees of practice, while their age ranges between eighteen and thirty-eight. It was thought advisable to test a fairly large number of observers, in order to eliminate individual peculiarities of color-vision, and also to get a comparison of the sexes. Our results seem to justify the precaution. The monocular data

[6] Several other observers began the experiments, but for various reasons could not continue. The writer takes this opportunity to thank each and all of them for their willing co-operation.

were obtained by placing a ground glass before the unused eye.

The detailed results are presented in the following Table. Each individual value for both eyes, for the right eye alone, and for the left eye alone is the resultant of eight series of determinations, while the figures of the rows headed 'totals' are the averages of the values for both eyes, and the right and left eye. The arrows indicate the direction from smaller to larger differences of the totals, and are inserted to facilitate the grouping of the observers. If we consider first the combined results of all observers,—in other words, the averages of the totals,—we find that the smallest increment necessary to distinguish between color and no color is required for our red, varying between 0.68° and 4.06° and averaging 2.23° ± 0.85°. Then follow blue, yellow, and green and this order agrees with that according to saturation, red being the most and green the least saturated of our colors. Since our green requires a limen three times as great as that of red, it seems reasonable to assume that its saturation is only one-third as great as that of our red, and, similarly, that the saturation of our yellow is about one-half of that of our blue, that of blue about five-sixths of red, and green five-sixths of yellow. These figures agree approximately with a number of estimates of saturation made by some of our observers as well as by outsiders, even to the point of individual differences; for two of our observers, who estimated the blue to be more saturated than the red, also gave a smaller limen for blue than for red. We do not wish to lay emphasis upon these quantitative relations of color saturation, because a larger number of colors should be investigated before any general conclusions can be derived from the experiments; but we believe that our method will lead to a reliable measure of saturation.

We may now consider the results of the nine individual observers. At first glance there seems to be little agreement among them; but a closer study shows that eight give a higher limen for yellow than for blue (although one of the eight shows an unappreciable difference only), seven have a higher limen for green than for yellow, and six give a smaller limen for red than for blue. The average difference between red and blue is 1.51°, between yellow and green 2.15°, and between blue and yellow 2.90°, showing that, on the whole, red and blue were more nearly equal in saturation than yellow and green, and especially than blue and yellow. The individual deviations from the general average are smallest in the case

TABLE OF ABSOLUTE LIMINAL VALUES FOR COLOR-SATURATION

Obs.	Vision	Red	Blue	Yellow	Green
C	Both	.81	1.25	1.31	1.69
	Right	1.00	1.19	1.37	1.87
	Left	.87	1.24	1.31	1.66
	Total	.90 →	1.23 →	1.33 →	1.74
		.33	.10	.41	
K	Both	1.31	5.28	6.62	7.37
	Right	2.06	6.50	6.87	7.88
	Left	1.81	5.63	7.62	7.63
	Total	1.73 →	5.80 →	7.04 →	7.63
		4.07	1.24	.59	
Lu	Both	.62	3.00	5.87	10.25
	Right	.62	3.00	6.25	12.12
	Left	.81	3.50	5.87	12.56
	Total	.68 →	3.17 →	6.00 →	11.64
		2.49	2.83	5.64	
Lo	Both	1.18	2.78	8.12	8.00
	Right	1.56	2.18	9.62	9.48
	Left	1.75	2.56	10.75	9.12
	Total	1.50 →	2.50 →	9.50 ←	8.87
		1.00	7.00	.63	
M	Both	3.25	4.62	9.25	6.25
	Right	4.50	4.00	10.00	7.12
	Left	4.43	4.25	11.00	8.32
	Total	4.06 →	4.29 →	10.08 ←	7.25
		.23	5.79	2.83	
G	Both	1.56	4.12	3.93	7.37
	Right	2.12	4.78	3.81	7.12
	Left	2.56	5.02	5.09	8.12
	Total	2.08 →	4.64 ←	4.28 →	7.54
		1.39	.36	3.26	
W	Both	2.16	1.56	3.09	5.37
	Right	3.19	1.37	2.59	5.75
	Left	2.87	1.12	3.12	5.63
	Total	2.74 ←	1.35 →	2.93 →	5.58
		1.39	1.58	2.65	
SG	Both	2.81	2.12	6.62	6.87
	Right	3.56	1.69	5.25	6.75
	Left	2.75	1.93	6.12	6.63
	Total	3.04 ←	1.91 →	6.00 →	6.75
		1.13	4.09	.75	
A	Both	2.27	2.12	4.62	7.66
	Right	2.43	2.00	5.12	7.33
	Left	2.62	2.03	5.62	8.12
	Total	2.44 ←	2.05 →	5.12 →	7.70
		.39	3.07	2.58	
Av. of all Obs.	Both	1.77 ±.75	2.98 ±1.24	5.99 ±1.88	6.76 ±1.55
	Right	2.36 ±.85	2.97 ±1.42	5.65 ±2.14	7.27 ±1.72
	Left	2.27 ±.86	3.03 ±1.39	6.28 ±2.34	7.54 ±1.93
	Total	2.23 ±.85	2.99 ±1.32	5.81 ±2.11	7.19 ±1.69
			1.51	2.90	2.15

of green, where there are only two extreme values; next follows yellow, then red, and finally blue, where the mean variation amounts to .44 of the average. So far as sex is concerned, the averaged results of the two women observers

are greater in the case of three colors than the general average, and in the case of green equal to it, while the seven men average better than the total with red and yellow, equal with blue, and slightly worse with green. Age seems to have no influence upon the liminal sensitivity; and, similarly, long experience with colors and photometric work showed no effect.

It was found, again, that the binocular averages are considerably lower than the total and the monocular results. In twenty-two out of thirty-six cases the binocular averages were lower; in ten cases the right eye, and only in four cases the left eye was lowest. The right eye was slightly superior to the left with all colors but red.

Our results cannot be directly compared with those of Aubert and with the other earlier work, because of the differences in the external conditions. But we tried to determine whether natural daylight, which, of course, is several times as bright as our artificial illumination, would bring about radically different results. The two observers G and K, with whom this comparison was made, gave the following results under natural daylight: red 2.37 and 2.17, blue 1.83 and 2.96, yellow 7.84 and 6.85, and green 5.84 and 6.25; or in words, both gave a much lower limen for blue, while for G yellow and green practically changed places and for K the limen of green was lowered. Since these experiments were carried on between 11:00 A. M. and 3:30 P. M. before a brightly illuminated window facing the shining mid-day sun, the yellowish hue of the light would, by contrast, appear more saturated and therefore reduce its limen, while for G it also considerably reduced the saturation of the yellow stimulus, presumably on account of adaptation, although for K no such effect was noticeable. The striking fact is that the limen for blue in natural light was lowered.

Finally we attempted to verify some of Aubert's results by using the Masson-Maxwell disc; but instead of projecting the color on black or white, we used a grey disc of the same tint as the color, while everything else was copied from Aubert's description. K saw the innermost ring of green definitely and constantly as a faint colored ring; the next ring was seen only barely colored, and fluctuated; the third was absolutely colorless, slightly dark, and unsteady; and the other three could not be seen at all. For G, the innermost ring was clearly colored green and always visible; the next was less definitely colored but was constantly seen; the third fluctuated and was barely seen as color; the fourth was visible only during very brief intervals and showed no color, being

just slightly darker grey than its background; and the other two were entirely invisible. The six small green squares which produced these rings were 5.5 mm and the intervening spaces 10 mm, thus corresponding from periphery to center to sectors of the following angles: 3°0′, 3°37′, 4°27′, 6°02′, 8°20′, and 14°50′. Accordingly the limen for G was 6°02′ and for K 8°20′; or, in other words, these values correspond as closely as can be expected with our previous data; but they are considerably greater than Aubert's figures. The difference must therefore be due to his use of black and white backgrounds instead of backgrounds of the same tint as the color investigated. The fact that both G and K could see a colorless ring slightly darker than the background seems to indicate that our equation of tint for color and grey as determined under artificial daylight was not absolutely true for the much higher intensity of natural daylight. The difference between color and background in Aubert's experiments must have been much larger, and hence may have produced colorless rings of light or dark grey which he may have interpreted as color-values; thus he may have obtained his lower limen. It must be left to further experiments to clear up the point.

We were fortunate enough to get a few observations on the color-limen from a color-blind person, but could not make a careful analysis of his defect at the time. His results afford, however, an interesting comparison with the normal averages. He gave the lowest limen for blue as 8.25° as against 2.99°; then yellow, 18° against 5.81°; then red 37° against 2.23°; and finally green approximately 140° against 7.19°,—but even then the color was named yellow rather than green. It may be mentioned in this connection that with every observer we had recourse to preliminary series for introspective descriptions of the liminal colors, the presence of after-images, and contrast-colors in the surrounding field; but the data thus far obtained are too incomplete to be included here.

ON THE INHERITANCE OF RHYTHM

By P. F. Swindle, A. M., University of Missouri

A. Introduction

It is perhaps unfortunate that the term rhythm must be used in this investigation, but usage seems to demand it. The objection to the term is that it may have three distinct meanings, physical, physiological, and psychological. The last of these concerns us here. There is certainly no danger of confusing the psychological with the physical rhythms; but confusion of the two organic 'rhythms,'—the physiological and the psychological,—is of common occurrence. For this reason it would seem well to point out the difference between the two by the following concrete example. The physiologist says that normal walking is a rhythmical act. Whenever a man is so badly paralyzed in one of his limbs that he is obliged to give the diseased member a peculiar swing in walking, the physiologist may use normal walking as a control and say that the afflicted man walks non-rhythmically. On the other hand, the psychologist ought to call the 'hobbling metre' movements of the paralyzed person a two-rhythm. The chief characteristic of *rhythm to the psychologist is the systematic accentuation and subordination of the elements of a series.* This interest at once eliminates all physiological and physical rhythms from the field of psychology.

Eugene Landry[1] has very well discussed the different conceptions of rhythm. His discussion was necessitated by his very broad definition of the term rhythm as 'la marche de l'énergie.' This would mean any activity is rhythmical where there is a succession of varying intensities. So he speaks even of the rhythm of prose recitation. He is not interested in all the intensity successions possible anywhere in the world, but primarily in the change of the energy of human action, and especially speech, from moment to moment,—in what is called psychological rhythm.

Views of rhythm, dating from early Greek mythology to the present, are based on the assumption that rhythm is instinctive. In earlier writings the hypothesis is explicitly stated, but in later editions there was apparently no use in stating a

[1] A bibliography is appended at the end of this paper.

theory so well known. The old assumption is never contradicted; and it is only by assuming the innateness of rhythm that one can justify the problems investigated and the attitude in general of the experimenters. There is only one other hypothesis that can be substituted for the old one, namely, that rhythm in general is not instinctive, or in other words, that it is the result of habit. If, with this assumption in mind, one undertakes a review of the literature on rhythm, one is bound to be shocked by the preponderance of pseudo-problems and by the great amount of attention that has been given to them. Perhaps it would not be out of place to examine a few of the most recent investigations in order that their contradictions may be shown, and also that the status of affairs concerning the problem of the origin of rhythm may be indicated.

Warner Brown undertook 'to determine which is the more essential to rhythmical grouping, the uniform time of recurrence or the uniform character of the thing that recurs.' The conclusion drawn by this investigator leaves the impression that if the phenomenon of rhythm is to be understood, it must be attacked from the temporal point of view. Lotze, Herbart, Wundt, and Titchener are supporters of this view. Brown speaks of *temporal rhythm* and of *accentual rhythm*. As a result of his work he is led to conclude that 'time aspects are fundamental and the accentual features, while necessary, are not at the root of the phenomena' because his experimental data show that 'on the whole, the temporal structure was maintained twice as well as the accentual.'

Another investigation is that of J. E. Wallace Wallin who was concerned with the extent to which the time of rhythmical groups may be varied without appreciably affecting the rhythm. Concerning this problem he concludes that: 'Absolutely periodic or regular occurrences are not essential to the appreciation of rhythm. To engender a feeling of rhythm always requires a certain amount of periodicity; but the margin of irregularity which may obtain is quite considerable.'

It seems rather remarkable that his conclusions should so explicitly contradict those of the first-mentioned investigator. It is in view of the following evidence that he drew the above conclusion. He was able to distinguish five grades or qualities of rhythm in relation to the amount of irregularity introduced between the beats of the rhythmical series. He speaks of five grades of rhythm,—excellent, good, medium, poor, and disrupted rhythms. The average amount of time-displacement or irregularity, in terms of percentage from the average, amounts

respectively to 6.36 (grade excellent), 8.53, 12.0, 14.5, and 17.8 (grade disrupted). In absolute units of time these figures amount to 0.0526, 0.0734, 0.0991, 0.1182, and 0.1488 sec.

Herbert Woodrow in a paper entitled 'A Quantitative Study of Rhythm' dealt with a two-rhythm and found that he could change it from an iambic (accent on the first syllable) to a trochaic (accent on the second syllable), and *vice versa* by lengthening either the time-interval following or that preceding the accent. By a like process with the three-rhythm he was able to pass from dactylic (accent on the first syllable) to anapaestic (accent on the third syllable), and *vice versa.* Other important facts found by Woodrow are the following:

(*1.*) The amount of rhythm as determined by the indifference-point may vary with the intensity of the stimulus. That particular point between two forms at which the nature of the second perceived rhythm is unpredictable, the author terms the indifference-point. This is the measure of the amount of rhythm. (*2.*) The amount of rhythm is found to increase with the rate.

Woodrow believes that rhythmical grouping is altogether a matter of temporal relations; and the effect of the accent in determining grouping is to produce temporal illusions through the over-estimation of the interval preceding the accent.

In another experiment, Woodrow discusses the 'Rôle of Pitch in Rhythm.' Here he employed the same method of measuring as before, and determined that giving one of the beats a pitch differing from that of the others does not influence the accent. Here, too, part of the paper is taken up with the consideration of the rate of recurrence of the rhythm elements. From the introspections of his observers he judged that the preferred rates of auditory stimulation range from 0.305 to 1.37 sec.

Much work has been done on the preferred tempo and there seem to be about as many different conclusions as there are investigators. For example, this tempo, according to Vierordt, is 0.62 sec; according to Stevens its lower limit is 0.53 and the upper limit is 0.87 sec; for Martius it is 0.50 sec; for Meumann it is 0.40 sec. These statistics, in the light of the theory of rhythm to which this present investigation is devoted, mean only that different people seldom make like movements with like speeds. The particular tempo which a person may choose depends in large measure upon the rapidity with which he ordinarily makes movements. This can be verified as I have done by correlating a person's reaction-time and his tempo. A high correlation is always present.

Another investigator who is generally considered to have contributed much to the understanding of rhythm is Bolton. He stated that it was his aim to reduce rhythm to a more fundamental activity of the mind. In the first part of his paper the author discusses all kinds of periodic movements from 'cosmic rhythms' to 'the incubation of fowls;' and in the meantime he shows how 'organic rhythms' have been decidedly affected by 'cosmic rhythms.' After this he talks about 'physiological rhythms,' which are no more than periodic recurrences of certain physiological functions. 'Of these, walking and speech are the most important and are true types of rhythmical activity.' All such movements he calls rhythms. These, however, are not the movements which he investigated; in his experiments he was interested chiefly in what he termed secondary rhythms. A secondary rhythm is derived from a rhythmical series of elements, according to Bolton, by accentuating one of the elements through increasing its intensity, pitch, or tone-color, etc., at regular intervals. 'Accent simply arranges the materials already rhythmical through some temporal recurrence.'

In speaking of rhythms in poetry, Bolton states that the number of accents to the verse may be four, six, or eight. The eight, however, fails to become popular because it exceeds the mental span. Because of the limitation of the mental span, the accents are limited to those two or three numerically small quantities; and it is only for the sake of variety that verses are made to contain five and three accents. Two, although it is the most primary rhythm, according to his way of expressing, seems never to have been used thus because of its extreme simplicity.

In ordinary life, groups of two things and multiples of two occur more often than three, hence rhythmic groups of two and four occur more often than of three. In a conclusion, however, the author makes the statement that 'a member of a sequence may contain *one* or *more* simple impressions,' which expression does not exclude any number.

The author raises the question of the inherent nature of a rhythmic group. The following quotation may serve to show his attitude toward this question.

'The conscious state, accompanying each wave of attention, groups together or unifies all the impressions that fall within the temporal period of the wave. As a result of a number of attentive efforts, a series of auditory impressions takes the form of a sequence of groups. This rhythmical grouping is due to the unifying activity of the mind. . . . . Each suc-

ceeding wave groups a like number of elements, so that the series is conceived in the form of groups. The rhythmical grouping is an attempt to conceive a series of sounds in a simpler form.'

It seems to be by means of this unifying activity of the mind that a series is transformed into a 'secondary rhythm.' The author's last statement, taken in connection with what he has said about the most popular rhythm, would mean that the four-groups and the six-groups are simpler forms than five-groups. If, however, all is dependent on the wave of attention, then groups of four and of six should be no more prevalent than five-groups. According to Bolton's theory of attention waves, it would seem that the five-rhythm must occur more often than the six-rhythm, since it is temporally shorter. From this dilemma his theory never escapes.

Stetson may be considered as beginning where Bolton left off. He is interested to know why the 'mind' possesses this unifying activity, or, as he would probably say, "why the human organism tends to divide a series of elements into unit groups." He is the first to see that the problem of rhythm is a problem of the activity of an organism rather than a problem of temporal consciousness. He is concerned with the causes of the organism's preference for certain groups of muscular activities,—why certain groups of particular numerical values are performed more often than others. Stetson was the first to mourn the fact that rhythm has always fallen into the hands of the investigators of attention, or the span of consciousness, or the perception of time. He says that not the temporal relations but the movements involved are the fundamental things to study if we intend to understand the phenomenon. He states further that it is a sheer assumption that regularity is characteristic of the pure rhythm, since it is easily proved that very wide irregularities can be introduced into a simple sound series without destroying the rhythm. He speaks of the human body as a device for producing rhythm. The larger muscles of the arm, for example, perform heavy movements, while the fingers execute finer movements. Repetition of this movement of the organ (the arm) naturally means a series of accented and unaccented elements of movement at *comparatively* regular intervals. "The unit group is the form in which the various muscle-sets and segments of a limb or organ can all work together, freely and easily, in a single movement cycle." The muscles of the arm can be spoken of as the major and minor muscles, the former performing the accentual movements, the latter making the finer movements.

Furthermore, combining several organs we find, for example, that the hand may make a series of movements which may be accentuated by a foot movement, or, perhaps, by a movement of the entire body. These movements may require a short time or they may require a long time. The speed differs with the particular nature of the parts of the body which move.

Stetson makes clear why two and three, including their powers and products, should be rhythms; but he does not explain any real distinction between five and three. The following quotation is his only attempt to explain why we often find unified movements (5 and 7 perhaps) in our experience. "The unity of an act seems to depend on the continuous character of its constituent movements and on the purposive habit which gave rise to it, rather than on the anatomical relation of the parts involved." But, inconsistently, he considers that five and seven are not rhythms. This may enable us to understand why he says that accentuation and subordination are '*perhaps*' the essential elements in rhythmic perception. He seems to believe that there is possibly another essential element hitherto unknown. If these two, accentuation and subordination, are the all-important factors, then why is it not possible to call five a rhythm quite as well as four? If four is a rhythm and five is not, then accentuation and subordination may not be of chief importance, because elements can be accentuated and subordinated to form groups of five just as surely as to form groups of four.

I am left to infer from Stetson's paper that he regards rhythm as instinctive. The essential element that is above referred to as hitherto unknown, seems to be regarded by Stetson as being of the nature of an instinct. He does not contradict the old view that rhythm is an instinctive something. Furthermore this is the only assumption that makes clear his attitude, viz.: "There is no reason for assuming that the nature of the unit-group of verse differs from that of other rhythms." "Most recent writers are inclined to reduce the types of feet to four; iambic, trochaic, dactylic, and anapaestic. All of the numerous kinds of feet occasionally given can be separated into these elementary forms." "The ordering of the unit-groups into larger unities is possibly a matter of historical development and might be studied in primitive art works."

With Robert MacDougall time is not such an important factor as it is with many other investigators. He concerned himself with many different problems, some of which I shall not discuss in this paper. I wish to speak of his work chiefly since he, like Stetson, superimposed his problems upon a hy-

pothesis of instinctive numerical selection of group elements. This hypothesis is made no more explicit than Stetson makes it; but I think I am justified in simply saying that he assumes, as does Stetson, that rhythm is instinctive. Both are concerned with rhythm as a particular form of activity peculiar to certain animals and not to others. In other words, they are concerned with the fact that human beings group their actions into certain unit groups; that human beings, therefore, possess this instinct.

MacDougall places emphasis on the fact that the human organism prefers certain numerical groups of movements to others. The preferred groups are spoken of as rhythmical. The other groups do not concern the investigator after they are once determined to be non-rhythmical. He makes no search for an organic law to explain why one group should be preferable to another. To overcome the difficulty he assumes, at the start, that rhythmical action is innate, thus assuming at once the right to exclude from the investigation all group-movements which are not innate, as being non-rhythmical. His non-rhythmical groups are the prime numbers higher than three. He does not explain the selective principle which causes the human organism to choose either a four or a six in preference to a five-rhythm. This phenomenon can be explained, as we shall see, only in terms of organic activity as dependent upon the arrangement of parts of our mechanism. MacDougall convinced himself that the seven movement is not innate (and is consequently not a rhythm), and substantiated his conviction by introspections from his subjects, who said that seven furnished no feeling of rhythm. He calls the movements of two and three simple and fundamental rhythms; the movements of four and six, complex and secondary; and the movements of five, seven, or eleven, no rhythms at all. One of these introspective statements is as follows: "The sense of equivalence fell off at five and practically disappeared at seven beats, while groups of six and eight retained a fairly definite value as units in a rhythmical sequence." He obviously means by this statement that the ability to accent every second or every third element is instinctive and that four, six or eight furnish experiences of rhythm because they have the instinctive rhythms as their bases. In performing the nine-rhythm the three is still performed; but each unitary group of three becomes one element for the secondary rhythm of three or nine. The nine is a rhythm superimposed on the three having as its elements the structural units of the three.

I have not at all attempted an exhaustive review of the literature on rhythm. I have only mentioned in a brief way

the contributions of some more recent investigators, as examples of those (the majority) who were chiefly interested in the time aspects of rhythmical action and of those (the minority) who subordinated this problem to others.

The contradictions noted in some of the preceding studies may not only cause one to be doubtful as to the usefulness of the primitive assumption (of instinct), but may be the cause for his becoming an aggressive skeptic. Let us now examine the following experiment which is designed to test the usefulness of our new assumption of rhythm as a habit.

B. Experimental

a. First Experiment

In this investigation I wish to concern myself with rhythm as a problem of organic activity of animal or human behavior. For some reason a characteristic of human beings is to divide repeated movements into certain numerical groups, or rhythmical units. A normal adult, if asked to beat a long succession of like strokes or to listen to such a succession of sounds manifests a tendency to group the elements into periods, that is, into successive unit groups numerically the same. The grouping factor may be an exceptionally extended pause, an exceptionally heavy stroke, a change in pitch, or any other means, although we need not assert that any or all would serve the purpose equally well. Rhythm is just this process of subjectively accentuating and subordinating elements of a series. If the grouping factor just spoken of, does not exist objectively, then it exists merely subjectively, as an illusion. I purposely speak of ***units numerically the same*** without implying true periodicity of time because I mean to lay particular emphasis upon the principles of accentuation and subordination, and to regard time relations of any definite nature as not essential. Time, of course, is important in rhythmical grouping since these movements must occur in time, but this is no less true of any other form of activity.

I hesitate to speak of time as being the chief factor in rhythm primarily for the following reasons. If it is the chief factor, why is it that in ordinary life the group of five if made at all, is performed with greater difficulty than the four, or the six, or the eight, but is produced with greater ease than the seven which in turn offers greater difficulty than the six, the eight, the nine, or possibly even the ten? If time were the chief factor, there would be no reason for such a preference of groups. Moreover, if it were the chief factor, the accent would be of no value. There would be no reason for making

a distinction between physiological and psychological rhythms. And this failure to make such a distinction is the very mistake Bolton made.

Our movements in rhythm must correspond to the normal movements of our organs involved. The fact that our large members require more time than the smaller ones gives a basis for understanding the function of the accent, or why it is insisted upon in rhythm. There are other things which contribute a meaning to the accent, viz., the fact that in ordinary life we find it quite necessary to make lightly certain tentative or preparatory movements before the real purposive action is strongly executed. Further, there is the fact that we are bilaterally symmetrical. This affords us two means for executing like acts. One member of a pair usually becomes subordinated to the other. One makes the strong, but possibly unskilled, movements and leaves the production of the finer movements to the more skilful member. It is in this way that right or left handedness (and "footedness") exerts its influence. Our inherited structure is such that in performing purposive actions that require the use of the two hands serially we must often make weak and strong movements alternately, or one hand make two movements while the other hand makes only one.

In ordinary life two and three and their multiples are regarded as fundamental rhythms, and five, seven, eleven, etc., as no rhythms. They are regarded thus by MacDougall. He did not raise the question as to the possibility of *making* the groups of five, seven, or eleven seem rhythmical. Since they were not rhythms they were not instinctive and were therefore not considered in the investigation. The question of chief importance in my investigation naturally follows at this point. It is: Are rhythms instinctive? And if so, what kinds? In order to have a working basis for this investigation, I have formulated, provisionally, an hypothesis similar to the one which MacDougall was led to assume in view of the same facts. My assumption was that the simple movements of two and three and consequently their multiples are instinctive, and that the prime numbers higher than three are no rhythms at all.

One method of solving the problem is this:—I first tested the subject's ability to perform repeated movements in groups of two and three and then his ability to perform in groups of five, seven, eleven, or thirteen. Of course, the error in performing more complicated movements was greater. If, however, by performing certain purposive actions that will necessitate groupings of five, seven, or eleven elements, repeated

movements of these numbers can finally be performed with as much ease and accuracy as of two or of three, the conclusion must follow that there is no evidence whatsoever for saying that the group-movements of two and three are instinctive and that those of five and seven are not; but only that our life activities, especially early in life, call for certain actions more often than they do for other actions.

The first apparatus used for the experiment consisted essentially of two parts, that used for developing the new rhythms, and that used in testing the subjects for rhythm. The apparatus used for developing the rhythm is a simple upright frame measuring 20″ x 20″. This frame carries four button bells on the four pieces of its structure, one above, one below, one to the right, and one to the left. Any button can be reached conveniently by a person who stands before the frame holding his hand near the center. Almost any type of purposive action can be performed by striking the buttons with a heavy, short-handled rubber mallet. Generally the purpose is to sound one or more bells in a definitely prescribed order. No counting is allowed while doing so: Take, for example, the five-rhythm. The subject performs an activity on the frame which necessitates a combination of four light movements and one extra heavy one to sound a bell. We need only to imitate a very ordinary complex of movements which occur very frequently in our every-day activities. In order to make certain that we hit a small object,—such as a nail head, for example,—it is ordinarily necessary to make certain tentative movements before the final stroke is made. This very common form of activity is employed here to develop rhythm.

For the five-rhythm only the two horizontal buttons, that is, to the right and left, were used. Let us hit first the left-hand button. The tentative activity is a simple complex of two movements, the easy stroke in the direction of the button and the back movement. Then comes the heavy and then the rebounding or second back movement. This leaves the beating hand very near the right-hand button which is to be struck next. One movement to the left is therefore a necessary adjustment. This completes our group of four small movements and one heavy one, mentioned in the order of two light movements, one heavy, and two light movements. The same, but reversed, complex follows for the right button, again for the left, again for the right, and so on.

The second part of the apparatus, or that part which is employed in testing, consists of two electric buttons to be struck with the mallets, a kymograph to record the strokes and an

exposure apparatus showing thirty different stimuli, which are a mixture of letters and numericals. The exposure apparatus serves the purpose of distracting the subject so that he *can not count* his strokes. The subject must beat each time, and must call aloud the name of each stimulus when it appears. The test as to whether any movement is automatic is the subject's ability to produce it while giving attention to something else. Rhythm is automatic when it is not a voluntary counting of movements.

The purposive action on the frame to develop the five-rhythm necessitates a combination of four light movements and one extra heavy one to sound the bell as just described. This continues for ten minutes. The subject is then tested to see if the special activity has led to an improvement over a previous test. In the test he takes a rubber mallet in each hand, and with the right hand (if he is right-handed, otherwise he will use his left hand) he strikes the electric button every time a stimulus appears on the exposure apparatus. In order to make the movements rhythmical he accents every fifth element. This is done by bringing more muscles into play, by striking with both hands simultaneously. This test would be valueless, however, without the function which the exposure apparatus serves, as stated above.

The particular rhythm movements with which this division of the paper is concerned are three, five, and seven. I had subjects ranging in variety from a high-school student to a university graduate. Some were more mature than others. For this reason I expected a number of variations to appear. My intention was to obtain such a large number of subjects that I might disregard such variations, and that at the same time my conclusions might not be limited to one particular type of individual. The work was begun with twenty-one subjects. Only fifteen of these, however, completed the experiments.

The results are as follows. All were tested for various rhythms ranging from the two-rhythm to the nine-rhythm. The experimental result of all previous investigations of rhythm was again manifest beyond a doubt, viz., almost all subjects could perform the movements of two and three as well as their multiples and powers provided they were not too large numerically, but very few could produce the five and the seven rhythms. Some of the subjects, however, had the five and the seven rhythms from the very first. It is interesting and important to note here that the subjects highest up in the scale have the shortest reaction-time. It is important

because it shows that the more active subjects are best. It is, of course, probable that the more active have the five and seven rhythms developed to a greater degree than the less active individuals.

In the experiment, the three rhythm, which is numerically simple and which is ordinarily considered to be instinctive, is used as a control. The average error made by twelve persons on this rhythm is 13 per cent. That made on the five rhythm after the first ten-minutes exercise is 24 per cent. (The lowest person on the five rhythm made 60 per cent, while the highest made only 1 per cent of error.) After the second exercise, the error for the five rhythm was equal to that of the three rhythm. We must not take this equality too seriously because the individual fluctuations are considerable, and the number of test persons is small. An absolute interpretation of this fact of equality would mean that five is as much a rhythm as three. At the next test, however, the error on the five is 15 per cent (or 2 per cent greater than the three), at the next it is only 5 per cent, at the next 6 per cent, and at the next and final test only 4 per cent of error.

By the heretofore prevailing hypothesis three is an instinctive rhythm. The 13 per cent of error made in producing the three rhythm is to be contrasted with each of the above figures.

Per cent of error on five-rhythm 24 13 15 5 6 4
Per cent of error on three-rhythm 13

The figures for the seven-rhythm (average of twelve subjcts) are as follows:

Per cent of error on seven-rhythm 23.5 11 16 9 12 5
Per cent of error on three-rhythm 14

Since some of the second group of twelve subjects were new there is a slight difference in the error made on the three rhythm. In general, the figures on the five and the seven movements lead to the same conclusions, viz., *if three is innate, the five and seven are also innate; if the three is acquired, the five and seven are also acquired.* One important thing to notice is the fact that very little practice is necessary to enable a person to perform the five or seven-rhythm even better than the three which is by hypothesis instinctive. There seems to be no reason for concluding that rhythm of any sort is instinctive, but only that generally in life certain movements are called for more often than others and hence become more automatic, more habitual. Our bodily mechanism is so constructed that in performing purposive actions, in work and

play, the movements have usually a particular numerical make-up.

In contrast with MacDougall's notion that five and seven furnish no feeling of rhythm I must say that I found just the contrary to be true, or at least the contrary effect was produced. Although I do not deny that MacDougall's subjects failed to have that experience I am convinced that this feeling may be found even outside the psychological laboratory. The composer Tschaikowsky wrote musical measures of five-elements obviously because he enjoyed experiencing them. The strongest evidence is that after the experiment was over certain subjects invariably made greater errors when the exposure apparatus was going slowly than when it was going rapidly. If seven furnishes no feeling of rhythm, then the subject should be more able to produce the seven-groups when the machine goes slowly than when it goes rapidly. When it moves rapidly the subjects must react much more or altogether from their feeling of rhythm. This could not be done at first, because the subjects in question had found no opportunity to develop the feeling. The same is true for the five as for the seven. Introspections also supplemented this evidence. I believe that considerable confidence may be placed in the introspections for I was careful at all times to keep the subjects from understanding the problem. I told them at the end. This eliminated the effect of suggestibility on the part of the subjects. It was difficult if not impossible for them to tell when they were making themselves agreeable, and when disagreeable. In other words, they were not in any way influenced by what they thought I expected of them.

In order to show that the increase in accuracy upon the faster appearance of the stimuli is not due to a happening upon a *desired tempo* (the desired tempo has been of considerable interest to a number of investigators), I need only mention the fact that the fast tempo was considerably faster than any of the subjects chose to beat *in the absence of the exposure-apparatus*..

Time does not seem to play an essential rôle. As to the preferred tempo I found that there was a wide disagreement among the individuals, and that some individuals preferred different tempos from day to day.

It might seem that the chief thing to be learned from this experiment is that the major premise of many previous investigators may profitably be replaced by a new one. If we give up the old notion that rhythm is instinctive, then the new conception follows as the only and as the reasonable

alternative. It appears at this stage more evident than ever that the more important problems heretofore investigated may be looked upon as pseudo-problems. If habit is at the basis of rhythm perception, the time element in all rhythm should be expected to vary. General agreement as to tempo would be truly remarkable. The time required to execute the movements, the time between the movements, the tempo, etc., should make such fluctuations as can be accounted for only when the nature of the environment that called forth the rhythm is well known.

I do not mean to say that all the previous investigations are valueless. The old hypothesis, conducive as it was to the preponderance of pseudo-problems, nevertheless furnished a background for the prosecution of some very interesting investigations.

b. Second Experiment

Let us now continue our old problem of the origin of rhythm, and at the same time try to determine *the most efficient means of developing* rhythm. The methods here used are somewhat modified. Instead of having certain persons work on one problem and certain ones on another, I had all subjects work on both problems. One subject beat the five-rhythm on the electric buttons and counted while so doing; then he was tested to determine the improvement which had resulted from this practice of counting. To develop the seven-rhythm the same subject performed the special activity of seven on the frame and then was tested. One case alone can not furnish evidence as to the part played by purposive activities in rhythm formation, so another subject, instead of practicing the five-rhythm by counting his beats and the seven rhythm by special activity on the frame, reversed the process. This arrangement was extended to all the pairs of subjects. Particular pains were taken to make the work a purposive activity which required no counting. This was done in order that the ordinary conditions of life might be approximated. In life we do not ordinarily count our movements, for example, in eating, working, playing.

We may look upon the experiment already described as preliminary. It gives a more comprehensive view to the problems with which the following portion of the investigation is concerned. In the present case, instead of having such a large number of subjects as before, I dealt with a few intensively. By taking an interested few I secured an ideal degree of regularity and promptness. Heretofore my subjects had no incentive to

come to the laboratory except that I asked them. With some of them promptness was entirely out of the question. Further trouble which hampered the experiment was due to the apparatus.

Besides many minor changes, the exposure apparatus was set aside and a new one was made. In general, however, the whole testing apparatus is essentially the same as at the outset. Aside from a number of minor difficulties, there are one or two others which are due to the nature of the experiment. One of the most serious difficulties consists in the fact that any adult can and wants to strike the electric buttons faster than he can recognize and call the succeeding members of a series of promiscuously arranged letters. It requires some time to adjust one's self to call a new letter. This condition means that the apparatus must go more slowly than the hand ordinarily makes such movements. The calling makes the difficulty. The letters can be recognized soon enough; but unfortunately this fact can not help matters since recognizing alone affords no objective test as to whether the subject is conscientiously attending to the letters and not perhaps counting his strokes. One remedy for the difficulty was to use only three letters,—*S, O, R,*—which were chosen because of ease in pronunciation. These were so arranged that no two successive letters were the same, but they recurred without any regularity. The second remedy was to enlarge the slot so that two letters could be seen at the same time. This enabled the subject to call each letter just as it arrived at a certain point, designated by side arrows, and just as a hand movement is made, and to adjust his vocal organs for calling the next. I desired that the conditions be such that the subject could not devote his attention either to counting or to analyzing the period units he is asked to make, into smaller units. It may be true that when a subject acquires the seven-rhythm he may be able to describe his experience as two groups of two and then one of three, or any other convenient combination of small units such as three, three, and one. If this be the case, the test to determine whether he has acquired the seven-rhythm would be absolutely worthless provided he is allowed to make the same groups during the test. It might only show me that he has a *quasi* seven-rhythm made up of the groups two, two, and three, this being a three-rhythm with the accent produced by an extra stroke. In the first place I tried to avoid counting while the rhythm was being developed. Then for the test I arranged the difficulties of the apparatus so that there was no possibility of even the subconscious grouping just mentioned.

For the small children who acted as subjects, the arrangement was soon found to be adequate to do this; but for older people it was not difficult enough and another device was necessary. The three numerals 2, 3, and 4 were arranged at promiscuous places on the disc of the exposure apparatus so that no two of the same kind appeared twice in succession. The promiscuous arrangement was such that after the appearance of a stimulus some (irregular) time elapsed before the next one appeared. During this time the subject beat to the usual click of the metronome and continued to accompany each beat by calling out with the same loudness the stimulus that had last disappeared. By this means all forms of number imagery which might have any relation to the rhythm and practically all other imagery except the kinaesthetic images of the rhythm movement itself were eliminated.

Several factors had to be taken into consideration here. The one of greatest importance was the fact that the hand can execute the movement more quickly than the vocal chords can pronounce successive letters or numbers after time has been taken to recognize them. To overcome this difficulty the numbers were arranged in large intervals so that it was not necessary to recognize them each time the metronome clicked; and too, the range of expectation was narrowed down to one or another of three easily pronounced numbers. Another factor which we encountered consisted in a considerable tendency on the part of adults to count and thus to perform a mere quasi-rhythm. The introduction of the numbers tended more than the letters to offer a decided interference with the tendency to count. In fact, I feel quite sure that the tendency was overcome entirely. The letter system was just as satisfactory for the children as the number system was for the adults. Children do not possess the tendency to beat so fast as older people; and when they are quite familiar with the three letters involved they can pronounce them almost as well as the adult. The more coördinated a person's movements become, the faster he wants to beat.

Four small children, of five, six, six, and eight years of age respectively, were tested for the movements two to nine inclusive. As the successive groups became numerically larger, a correspondingly greater error was manifested. This increase in error was a gradual one. There was no sudden increase at five and seven as was observed with the adults. In fact, the error for each of the possible rhythms was very great, so that there seems to be little justification for saying they have any rhythm other than the two. The greater prob-

ability of the chance occurrence of the small group, two, in their life activities is possibly responsible for the small error made as contrasted with the larger groups. Two of the children did not continue the exercises long enough. The numerical results of the following tables refer, therefore, only to the other two.

The ability to perform the five-, six-, and seven-rhythms should be contrasted with that of three months previous.

	V-rhythm Errors	*VI-rhythm Errors*	*VII-rhythm Errors*
The five year old.....			
1st	88 per cent.	85 per cent.	90.5 per cent.
2nd	46 per cent.	64.5 per cent.	60.0 per cent.
The six year old.....			
1st	82.5 per cent.	84 per cent.	88.5 per cent.
2nd	69 per cent.	80 per cent.	40 per cent.

There is no marked deviation from the results of the first test; but for the second test after three months of practice one significant fact is manifest. Let us consider the two individual results thus:

	V-rhythm Errors	*VI-rhythm Errors*	*VII-rhythm Errors*
H. Five year old (error in second test)....	46 per cent.	(64.5 per cent.)	60 per cent.
D. Six year old (error in second test)....	69 per cent.	(80.0 per cent.)	40 per cent.

(1) The error made on the six-rhythm is in each case greater than either the five or the seven-rhythm. *The only possible exercise here to develop this rhythm during the investigation is the mere beating on the buttons for the test.*

(2) On the five-rhythm H. made a smaller error by 23 per cent. *H. used the purposive method to develop the five-rhythm while D. used the counting method.*

(3) On the seven-rhythm H. made a greater error than D., by 20 per cent. *Here H. used the counting method and D. used the purposive method.*

The exercises were taken twice each day, as nearly as this could be done during the three months. The periods of exercise for each subject were five minutes in each of the three methods. Pains was taken to make the conditions of work for the two subjects identical. They worked during the same times of day, for the same periods, and were tested during the same hour. A little more might be said about the three methods just spoken of. *1.* The test method consisted in the rhythmical striking of two buttons on the table, the unaccented

strokes made on one button with one hand only, the accented strokes on both buttons simultaneously with both hands, with counting excluded by the requirement of recognizing and calling out the stimuli exposed at every beat, as described. *2.* The counting method consisted in rhythmical beating just as in the test method; but counting was required, and the exposure apparatus was, of course, out of function. *3.* The purposive method consisted in performing one of the specially chosen actions on the frame. For the five-rhythm, this activity has been described. For the seven-rhythm, it was the one corresponding to the following task: Strike the button to the left lightly (first movement); move to the right to suit the rebound (second movement); make a preparatory movement of adjustment to the left (third movement); strike the button to the right lightly (fourth movement); rebound as before (fifth movement); make a preparatory movement to the right (sixth movement); and now make a heavy stroke in space stopping suddenly near the center of the frame, to be followed by all the inner muscular adjustments (seventh movement, period completed) necessary to perform the same action in the vertical instead of the horizontal plane. Perform the same action again in the horizontal plane, again in the vertical, and so forth. Such a purposive action is comparable to that of a workman serving a particular machine in a factory, where, of course, he would not count, since the task can be done perfectly without thinking of any numbers. No numbers are found in the above description of the task. Similarly, what the subject in the laboratory has in mind does not include a series of numbers, but is simply the idea of the purpose, of the prescribed task.

It now remains for me to tell a part of the story which the tables do not show. Let us take a particular case. H. had acquired the five-rhythm so well that in the test he was making no errors when suddenly he stopped to laugh about and tell me how his dog 'Jack' went down stairs with all of his legs bent back. For a short time after this pause no particular movement was executed until he happened on the seven (which was by this time a rhythm) when he beat five groups of seven elements each. Ordinarily when H. happened on the seven, he usually made quite a series of seven-groups; but if he should be beating the seven-rhythm when the confusion occurred he would very likely start the five, and in that case he would beat a much longer series of five-groups than the seven-groups in the reverse transition. The same general statement can be made of D. except that she (since the seven

had become easier for her) would beat the seven for a longer time than the five. Toward the latter part of the three months, I could always start the children beating the rhythms five or seven by working their arms. I could not induce them to beat three-, four-, eight-, or nine-rhythms by the same process, or any other, for that matter. As soon as I released their arms they would very soon begin beating either the five or the seven.

After a lapse of five months, H. and D. were again tested for the rhythms two to nine inclusive. Those rhythms which were learned by the purposive method were reproduced even better than at the previous test. At this test there seemed to be nothing new concerning the other possible rhythms, except that those which had been learned by the counting method, and the six-rhythm, were performed but slightly better than before any practice had been acquired. What little had been learned about them seems to have been forgotten almost entirely.

The subjects were again allowed to practice. This time only the purposive method was used since the relative inefficiency of the counting method was quite apparent; and as usual H. developed the five-rhythm and D. the seven-rhythm. At the close of seven exercises of about twenty minutes each they were able to produce the rhythms by making only an occasional error.

c. Third Experiment

Similar experiments with adults and with more complicated apparatus for adults furnished even more striking confirmations of the conclusions to be drawn from these experiments with children. More difficult rhythms were here taken into consideration. One person practiced the thirteen-rhythm on the frame, and used the counting method for developing the eleven-rhythm, while a second person reversed the process, that is, beat the eleven-rhythm on the frame and counted for the thirteen-rhythm. After seventeen periods of practice *each of these subjects made very little error in producing those rhythms which were developed by the purposive method, that of beating on the frame. On the other hand, no evidence could be found that rhythms were developed by the counting method.* It was found that instead of beating the rhythm which he had tried to develop by the counting method, the subject beat either no particular rhythm or the one learned by the purposive method. This outcome seems truly remarkable, especially since in the experiment with the small children there appeared

to be evidence to justify the use of the counting method. The adults *not only succeeded in producing those rhythms* which they had learned by the purposive method, but they were *conscious of almost every mistake* made during the tests. These mistakes, however, were so few that in one case the error was 5 per cent, while in the other it was 7 per cent, the greater error being made by the person who beat the simpler rhythm, viz., the eleven-rhythm.

C. Practical Applications

In view of these results it may be interesting to discuss the " Rhythmical Gymnastics " of Jacques Dalcroze. Dalcroze is a teacher, formerly at Geneva but now at Dresden. His plan is to train his pupils to produce rhythms well, before they begin the study of music. The pupil's training is identical to what has been called *the counting method.* Dalcroze and his followers are convinced that great benefit is being wrought by this method, but to substantiate this conviction they have resorted to mere opinion. No test has been made to determine whether or not the rhythms were actually acquired by the Dalcroze method. It seems reasonable to suppose that the Dalcroze method is not altogether fruitless. The last described experiments on adults which seem wholly to discredit this means, contained only the small number of seventeen exercises. If this number were considerably increased it is possible that something would finally be gained even by the counting method. At least since this was the case with the small children there is no reason to believe that the exercises given by Dalcroze to his pupils are entirely fruitless. The writer is forced to the conviction, however, that the purposive method or method of ordinary life is the more efficient one.

The purposive method eliminates all forms of imagery except the kinaesthetic images of the movements. In the counting method, the analysis is not so complete; for by it are developed two forms of imagery, viz., kinaesthetic and number imagery. There are many activities which demand kinaesthetic imagery, but not number imagery. (My test may be cited as an example of such an activity.) For a person who possesses both kinds of imagery, the test is markedly a new activity, whose performance necessitates the elimination of the number images. It is an easy matter to inhibit these, but such an inhibition seriously disturbs the rhythm. To a certain degree the rhythm must be relearned. On the other hand, the purposive method fits the person for any activity which involves the rhythm previously learned.

Learning to perform, by means of one or many different members of the body, a purposive action of *n* movements of which one is "bigger" than the (*n*-1) others and being then able to tap on the table in such a manner as to accentuate every *n*th tap, involves obviously a *transference* of an identical form of activity from one part of the organism to another. The extent to which and the ease with which a well-learned rhythm may be carried over from the hands to the speech organs, for example, and back again to the hands with the result that one has difficulty in recognizing it, is worthy of note. The writer was able to perform the six-rhythm without error, in the ordinary way of beating five times with one hand and then once with both. After half a dozen ten-minute exercises, the same rhythm could be beaten accurately in a new and more complicated way which will at once be described. The training which is necessary to bring about the transition is the following. Take this "sentence" of six simple words. *I I can not go go.* These six words take the place of the above six beats. While these words are spoken, the right hand beats three times while the left hand beats only twice. The right hand beats to the first *I,* the *can,* and the first *go,* while the left hand beats to the first *I,* and the *not.* In musical terminology this is a "triplet," that is, three notes played with one hand in the time of two notes played with the other hand. This activity is deceiving, for it seems as though the human organism is producing a three-rhythm with one member while with another it is simultaneously producing a two-rhythm. Careful consideration of the above method of acquiring this ability, however, reveals the fact that this is not the case, but that this is only an example of the six-rhythm with embellishments added. Instead of first one hand and then both as in other cases, each hand has here a different share of the six movements to execute. Where the hands fail to execute one of the elements of the six-rhythm, the muscles of speech execute it, and complete the six elements of the period.

The 'quintuplet' of the form five to three was learned by people in a similar manner. We employed the following sentence, which contains fifteen elements. *And now you may see*

I've crossed the big sea and got to New York. The letters

R and *L* below the sentence indicate the words accented by the right hand and those accented by the left hand. In either

case, these movements of the hand are real accents (secondary, of course, to the one main accent of the whole sentence), since each brings into play more muscles than the muscles of the speech organs which function with every syllable. Only the very superficial observer would describe this process simply as the production of the five-rhythm with the right hand and the simultaneous execution of the three-rhythm with the left hand; for this process is really the fifteen-rhythm.

The method of the previous experiment was originated by Mrs. Fannie Church Parsons of Chicago and applied to kindergarten children. There seems to be, however, no reference to her work in the literature. This method does not develop needless number images, but it does develop, in addition to the kinaesthetic imagery, visual and auditory images of the words of the sentence. These latter images may be useless, and perhaps they are a hindrance in such an activity as my test involves. Fortunately these images do not stand much in the way since they can easily be inhibited without seriously disturbing the rhythms. This system, therefore, has decided advantages over the counting-method and may be comparable in efficiency to the purposive method as previously described.

The Dalcroze method of teaching rhythms has been referred to in this paper as a *counting-method*. Some explanation is here necessary since Dalcroze says: "Ich verlange garnicht, dass man zählt. Es sind die Unbegabten welche zählen. . . . Meine Methode ist Sache der muskulären Erfahrung." ("I do not require any counting of my pupils. It is the untalented who count. . . . My method is a matter of muscular experience.") The hopes of Dalcroze are here plainly stated; but whether he fully realizes them in practice is open to question. In one sense his statement as to the untalented is a confession that counting is by no means discouraged by him or made unnecessary, for his pupils can and do count. I have spoken with a number of people who have acquired rhythms by his method; and the following stubborn fact is always to be observed. In order to reproduce the rhythmical actions learned under him, which during the course of time have almost been forgotten, they invariably count. Number images are the means for reproducing the movements. This shows that in his teaching Dalcroze has not succeeded in making the 'muscular experience' independent of number images. I have no reason for supposing that these people with whom I have spoken are untalented. I rather think the method is such that all have an inducement to count. Perhaps still another quotation is here appropriate. "Wenn man den Rhythmus seiner

Glieder empfindet, braucht man nicht mehr zu zählen." ("As soon as one perceives the rhythm of his bodily members, one does not have to count.") So it is evident that the students start out by counting; and no one but the pupils themselves can say whether or not they continue to count throughout their training. It is no wonder, then, that counting is the means for reproducing the old movements. Under these circumstances I doubt if Dalcroze's method of teaching 'rhythmical gymnastics' deserves to be regarded as a specifically new method. His success as a teacher seems to be due to personal qualities rather than to excellence of method.

Dalcroze says plainly, "Ja sicher, Rhythmus ist erblich." ("Yes, surely rhythm is hereditary.") If the rhythms are inherited, then one method is as good as another to describe what action is at any particular time desired. Why, then, not use counting since it is the simplest means? But according to experimental results, included in this paper, the avoiding of number images seems to be a matter of expediency. Dalcroze is aware of the possibility of eliminating counting after the rhythms have been, so to speak, found by the students. He conforms to natural conditions in so far as he takes pains to allow (large) movements of small frequency to be made by large body members and (small) movements of greater frequency to be executed by the smaller members. Thus it is possible for him, after persistent and consistent work, to emancipate himself from all counting. This conforming to anatomical conditions, as just stated, seems to be the whole of the 'Dalcroze method.'

D. Summary

All experimental evidence seems to point to the conclusion that rhythm is acquired by each individual, and that it is not inherited. Biological conditions,—for example, the anatomical fact that we are two-footed, two-handed, and generally two-sided, not three-cornered or star-fish like beings,— are favorable for the development of those rhythms which have usually been considered to be instinctive, while the other rhythms can be acquired only under special, somewhat artificial conditions. The best means for developing rhythm is that which approaches our ordinary life activities. In the development of a rhythm, the motor activity of the skeletal muscles plays the most important rôle. For this reason, the larger movements of a purposive activity are much more conducive to the production of rhythm than the smaller movements which accompany the almost purposeless activity of the counting method.

I feel greatly indebted to the many people who persistently acted as subjects in this experiment; and I am especially grateful to Professor Max Meyer who has been ready at all times to advise and assist me, and who even devoted much of his time to serving as subject himself.

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KINESTHESIA AND THE INTELLIGENT WILL

By George Van Ness Dearborn[1]

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I. Introductory

The purpose of this research and essay is to point out in terms of more or less well-known nerve-circuits a new consideration or two as to the nature of human motivation and will. In the chosen title the term intelligent is used, therefore, in a very broad sense to include several processes other than intelligence in the technical psychological usage, particularly affective tone. A more precise expression than intelligent will would be perhaps teleologic conation, however cumbersome such a term may be. Both, however, seem to the writer to express definitely enough the empirical motivation-tendency of the actual human adult.

Just as, psychologically considered, vision is undoubtedly the "queen" of the senses, so physiologically the processes inherently relating to movement, posture, weight, spatiality, etc., are assuredly the most important. In the universal integration of sensations, vision in a way may even be considered the mental homologue of the bodily kinesthesia as a little thought readily shows. Only now are educators beginning to realize the indispensable usefulness always and everywhere of kinesthesia, the "feelings of movement." Kinesthesia is about, however, to come into its own as the primary and essential sense. Without it, coördinated and adapted bodily movement and strain, concomitant to every kind of mental

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process, is inconceivable, for the (psycho) motor centers in the brain have no known clairvoyant powers and therefore their function of carefully coördinating the distant muscles, *e. g.*, of foot or hand, is entirely dependent on their continual reception of detailed information as to the relative tonal and contractural status of all the active parts to one another. Simple as this idea is, the immense practical importance for education has as yet hardly begun to seep into the effective minds of educators. Its relation in a general way to voluntary movement has recently been set forth inadequately in an article (17) by the present writer from which the following quotation may be expedient.

"The *nervous circuits* which underlie voluntary movement are really of course the functional framework, so to say, of all neuro-musculo-glandular activity. The idea is the modern successor of the reflex arc, the succession being made necessary by our increased knowledge of the nature and uses of kinesthesia. Here again our classification is arbitrary to some extent, but none the less useful, perhaps, for descriptive purposes, and withal tentative.

The simplest "circuit" that we have to consider is that which we may call intramuscular. The knee jerk, for example, is far too quick to be a spinal reflex, so that we have to suppose it a direct reaction from the thick subpatellar tendon of the muscle-mass concerned, a direct stimulation of the elastic muscle-fibers. It is obviously only for the sake of logical completeness that we start with this, for a literal circuit here is hard to define, and moreover useless for our present purpose.

The next type of circuit is similar to the vegetative circuits of the sympathetic system, such for example as are concerned in the movements of the intestines, by the plexuses of Meissner and of Auerbach. A third type is of a partly reflex and partly sympathetic nature, and represents those reactions that are primarily reflex and yet in part under the control of the sympathetic system. A good example of such a circuit would be found in an attack of painful cramp from distention of the gut. This would constitute for the purposes of our discussion an example of a circuit intermediate between one purely vegetative (under the entire control of the sympathetic system) and an out-and-out reflex. A fourth kind is the typical reflex, termed "epicritic." Other circuits go up to the bulb and may be termed medullary circuits, and comprise those that have to do with respiration and heart-action and many of the other vital functions which are apparently controlled by the medulla. The

next variety of circuit we may term the nuclear circuit, such as those that go up into the big neuronal masses in the interior of the brain and whose activities in part at least are of the type of emotional reactions.

Lastly, the most complex type is that properly known as the cortical circuit, and this especially is accompanied by consciousness and, more important still, is under voluntary and initiative control. It is an important circumstance that each one of the circuits mentioned includes more or less of the apparatus of all of those below it. Thus you have a series ever enlarging upward. Anything that might happen in the lowest circuit would more or less affect the higher, and anything coming from the highest would have all the other circuits more or less under voluntary control, unless prejudicial to efficiency. The highest and longest and most complex circuit, the top one only, then, properly speaking is *voluntary* in nature.

These circuits of nervous influence, as various as their unique routes through the central nervous system, are not represented, of course, by separate neurons. Again, some of them are of actuating and some inhibitory nerve impulses, some produce action while others inhibit it, in some cases not only stopping the contraction of the muscles but actually and actively relaxing them. Sherrington especially reports an experiment upon the reflexes in dogs in which active relaxation of the mucles occurred. We need to know more of this! Inhibitions in the adult have long since reduced themselves largely to the reflex type. Moreover, the inhibitory impulses by their nature are negative and have been lost sight of, so that oftentimes we do not realize that they are present at all in the adult individual. For example, the very essence of the action of alcohol upon the finer cortical centers in the brain is an inhibition (depression?) and yet, as we all too well know, the moderately intoxicated person shows signs of anything rather than that of inhibition or negation. It needs emphasis here that in general the part played by inhibition in voluntary movement, as also in attention (13) is preeminent. Inhibition, however, is too little known still to allow of its discussion at this time in this complex connection. The work of Nikolaïdes and Dontas (confirmed by Wooley and Fröhlich) demonstrating actual inhibitory fibers in the muscle nerves, is a recent important step along this path.

There is a classification of the nervous circuits we have mentioned other than that of actuating and inhibitory, and it is one that is of more immediate use in the theory of

voluntary action, namely, their division into circuits on the one hand between the skeletal muscles and the cord and into those, on the other hand, between the grey cord and the grey fabric of the hemisphere above.

As to these former nervous impulses: Few facts more helpful to our knowledge of personal control have appeared in late years than that the muscles of the body, anatomically but not physiologically individual are mainly coördinated in the posterior grey horns, these being subordinated in some certain directions (equilibrium, resistance-purchase, etc.) to supervision from the cerebellum. As a corollary of this, important very to our present intent, it is clear that the separate muscles have no direct representation at all in the cerebrum proper.

The truly personal control, then, of the cross-striated musculature comes through influences of the second variety just suggested—namely, those between the spinal horns and the grey fabric of the hemispheres, cortical and nuclear. This control, moreover, is certainly "symbolic;" that is, a properly adapted single influence probably controls a whole movement, although in the spinal grey this may involve the coördination of very many muscles in a functional group. Moreover, this control as it comes into these muscle group centers of the cord, is a *resultant* of numerous complex factors which it is the business of the hemispheres to produce by the stress and the strain, by the thrust and the pull of the individual psychophysical conditions at the moment or constitutionally, or both. It is the resultant, apparently, that controls the grey cord's group action.

One of the most essential of the factors in the neurility of voluntary control we now all recognize as kinesthesia. We do not, of course, need to enlarge upon its nature here, for Goldscheider, von Frey, James, Bastian, and many others have made its organs and its preëminently important functions in the conduct of life common knowledge manifest to all, details, however, being added continually. These kinesthetic impressions or impulses come from the moving parts into the grey matter of the hemispheres. Take the elbow or the wrist joint, the fingers, or the shoulder joint, and consider all the scores of muscles, tendons, and bones involved, as well as the skin over these moving parts, and include also the sense of touch, and we can see what an enormous complex of kinesthetic sensations and impressions must crowd continually into the central nervous system from every part of the ever-moving body.

The familiar work of Mott and Sherrington on the afferent spinal roots in dogs was quite conclusive as to the status of these movement "sensations" or influences. It will be recalled that the efferent nerves in these experiments were left complete, the outgoing motor neurons, but the dogs none the less were quite incapable of making any efficient voluntary movements, properly speaking; Munk then took up this work of Mott and Sherrington and demonstrated that the dogs after a time could make voluntary movements, but that they had to learn to do so by the vicarious use of their eyes, these organs taking up symbolically the functions usually ascribed to kinesthesia. Experiments upon certain paralytics who lack the power to make a voluntary movement show this same fact of vicarious symbolism to be true. It is sufficient to say, in short, that the kinesthetic impulses from the moving parts of the body start, or at least direct and control the voluntary movements of the individual considered as mechanical events. Unconscious kinesthetic influences appear to direct the gross movements with the help of vision, while the conscious muscle-joint impressions control the fine adjustments by an inhibitory mechanism until they, too, have become subconscious by habituation. Whether we believe with Bastian or with his opponent Ferrier as to the topographical nature of the great cortex, we cannot fail to see that the circuits between the muscles and grey cord and brain and back again are at once *the framework and the substance* of the neural process in voluntary movement, the former half of the circuit in each case being kinesthesia, be the details what they may."

II. Research

The writer is enabled to add something to the physiology of kinesthesia as the product of experimental work among his students and colleagues in the Tufts College Medical and Dental Schools and in the Sargent Normal School. This research deals directly with the functions of the nerve impulses concerned in the so-called muscular sense. The investigation was made by voluntary movements; but its results are equally well applicable to other muscular coördinations. The nerve processes that determine voluntary movements have been much discussed of late by the psychologists especially and have enlightened us not a little, while the neuro-histologists and the clinicians have been clearing up their side of the neurology of voluntary movement somewhat. In a sense, the present work forges the links connecting these two chains of information.

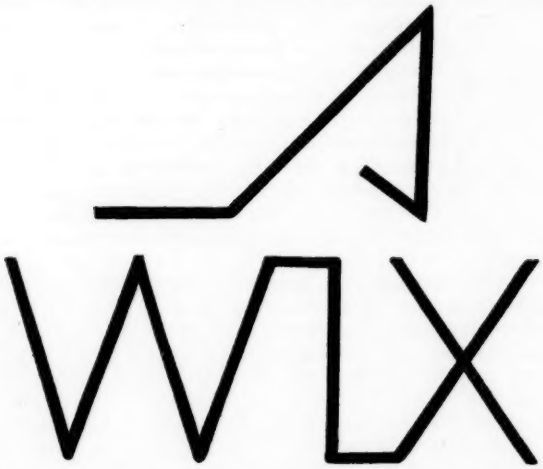
One obvious reason why progress has not been faster on the introspective side of the problem is that most of these experiments (74), *e. g.*, have studied rhythmic movements, movements made with voluntary muscles to be sure, but yet largely lacking the characters which distinguish voluntary action from that which is already more or less habitual and organized. An ideal voluntary series for such an investigation would be the movements of a novice learning to engrave without mistake a deep monogram on the side of a silver cup, or those of a youth learning to cast a small trout-fly in a steep and narrow rocky river with alders along its banks and with old beeches overhanging. Such behavior is action that is truly voluntary,—but quite as truly insusceptible of systematic scientific measurement and study. Again, we certainly can get but little information as to ordinary muscular control by the training of abandoned (75) or even degenerate muscles, such as those of the pinna. The present experiments seek to avoid both of these apparent defects by using muscles that are under excellent personal control in movements at once simple and yet, as wholes, new to the individuals making them. From such easily defined and truly voluntary movements it was hoped that useful kinesthetic information might be gleaned.

The writer has already, in several places (17, 18, 19), expressed his opinion as to the general nature of the cerebral influences determining voluntary movement (a result already reached, by the way, unbeknown to the writer, by Woodworth),—namely, in a word, that the determinant can be nothing less than the whole disposition of the brain and organism at that particular moment,—cord, sympathetic, and all. He may not, therefore, be classed with the ultra-kinesthetists who overestimate the importance of these movement-neurograms for bodily management. None the less, as everyone must admit who realizes this problem of nerve-control over thousands of otherwise unconnected muscle-units, kinesthesia must be seen to constitute the chief spinal factor of muscle-coördination, and so of voluntary control.

A. The Apparatus and the Material

This research was begun in November, 1910, and has been prosecuted at intervals since, on thirty-eight subjects, all of good intelligence but varying in age from fourteen to sixty, of both sexes, and some of them blind, the majority of them were young women between eighteen and twenty-five years old.

At first, a rather elaborate electrically recording apparatus, devised for the purpose, was employed; but this was abandoned for a method at once simpler and more efficient, and moreover closer to the actual conditions of motor training, to the knowledge of which these experiments are a contribution. The apparatus chiefly employed, then, was of the simplest,—two norms or models, shown in the figure, these being simple series of connected straight lines traced on a sheet of paper. One is a horizontal line 2 c. m. long thence 5.25 c. m. upward at 45°, then vertically downward 3 c. m., then 1 c. m. upward and backward (to the left) at an angle



of 45°. The other norm, as may be seen from the figure, is really a W, an I, and an X connected by short horizontal lines, the whole being 8 x 3 c. m. overall. In addition to these norms the apparatus comprised only a full-length lead-pencil, sheets of white paper 21.5 by 28 c. m. in size, and iron clamps to hold these sheets to the corner of a table, sometimes under a sheet of plateglass.

B. The Method of Research

The method of conducting the experiments was equally simple, as is always fitting if we are to get to the bottom of complex things. The subject, carefully blindfolded, stood before the table, was given a long pencil, and told that having

been given "a certain series of straight lines" he was to "draw it on the sheet of paper before him as accurately as he could, both in size and shape." The pencil being held with sufficient firmness in the subject's right hand, the experimenter with a hand on either end of the pencil then guided the point of the latter at a moderate speed over the norm. The subject was then asked if he could repeat this voluntary movement, if he could draw what had been given him kinesthetically. If he had a sufficiently clear idea of it, a sheet of paper was placed under the pencil-point and the first record was made. Information was then requested as to the nature of the motor idea by which he had been guided (whether visual, kinesthetic, or otherwise), and the notes were written upon the sheet. Usually more than one kinesthetic perception of the norm were necessary before a sufficiently clear notion of the series was obtained; the subjects varied very greatly in this respect. After several repetitions of the voluntary movement by the subject, he was asked about the movement-feelings in his active arm and his replies were carefully set down. He was then requested to notice them as intently as he could and to concentrate his attention upon them, and meanwhile again to draw the series of lines as before. Careful inquiry was made in every case as to the success and other conditions of this process of intensification of the resident kinesthetic sensations and the degree of success, with the other notes, recorded. After a few performances alternating between the natural method and this one with strengthened movement-sensations, the blind was removed from the eyes and the norm was visually shown to the subject (no measurements being allowed), and the blind was then replaced. Note was made as to whether the visual percept seemed surprisingly large or small or neither. The former alternation of visual and kinesthetic guidance was then continued for a few performances, real intensification of the resident sensations being assured, as before, by suggestion and unobtrusive insistence. Finally, the blind was removed, and the subject was asked to draw the norm as well as he could with his eyes open, as a final memory-standard, based on all his combined experiences of the series.

Thus we have five sets of similar voluntary movements for qualitative and, if expedient, quantitative, comparison: a set made from a motor idea kinesthetically received; another on this same basis but with intensified movement-sensations; a third, made from a motor idea both visual and kinesthetic in origin, another, on this latter basis also but with the move-

ment-sensations reinforced; lastly a set of performances made with the eyes open on the basis of the previous experiences combined. Here obviously is a wealth of material for the study of many things,—for example, the effects of intensification of the conscious phases of kinesthesia, which we use, and data for fundamental knowledge as to individual differences, which we postpone to another time.

A third but brief series of experiments was conducted on some of the subjects in the drawing, blindfolded, of 40° angles with sides about 20 c. m. long, and received (perceived) by the performer kinesthetically as in the other experiments. This work was soon abandoned for a better plan. It was, however, here brought out without chance of error that difference in direction has no primary, *natural* basis in the kinesthetic memory of visualizers, however, easy of development there. Differences in direction depend on visualization, whether it be the literal process as used by most people who see or that close imitation of it, derived probably from kinesthesia plus local signature, so conspicuous in the minds of the congenitally blind. There is assuredly some elaborate process of synthesis that is readily competent to give human beings their necessary working concept of spatiality even out of the most fragmentary experiences; that it readily arises from kinesthesia can be marvelled at by no one. The necessity for this spatiality has been discussed elsewhere, for example (37, 18, 17).

C. Notes and Results

The experiences concerned with the performances in these experiments were clear to every subject and are of such a nature as to be clearly statable by every adult of normal intelligence, for movement could not voluntarily eventuate until its conditions, in part at least, were known. The research is as much neurological as physiological and more of either than psychological, for no one can divorce in such conditions the facts of personal experience from the nerve-impulses underlying them; and herein can neurology receive more help, oftentimes, than from the histologist or ever from the clinician. The number of subjects is sufficiently large to ensure reliability; and their variety in age, sex, and race, and both blind and 'seers,' adds to this trustworthiness. The work endeavored to keep as close as possible to the ordinary conditions of average voluntary movement with the arm; the chief departure from this normality was obviously the blindfolding of the subjects, but herein lies of course the crux of the results in a study of kinesthesia. It is one of the unexplained curious facts of

psychology that in unskilled movement, vision completely drowns out the kinesthetic sensations. The lines of the norms (one simpler, the other more complex and requiring better memorizing than the other) are about of the average length of voluntary movements, being much larger than the movements of the writing class, yet greatly smaller than the free arm-actions so common in ordinary behavior. This latter consideration is one of no little importance in judging the protocols of the experiments for in a free right-arm movement such as these, made with the individual standing, the continual tendency would be to exaggerate the movement beyond its actual small extent.

Of the results of this research, perhaps the most interesting for the physiology of kinesthesia is the fact that reïnforcement of the conscious movement-sensations by deliberate attention to them tends to shorten the movements and somewhat to lessen the angles between them. In other words, in visualizers, intensification of the conscious kinesthesia inhibits the related voluntary movements. Such being the case, it is difficult to avoid the conclusion that *the function of the conscious movement-sensations is the inhibition, the deliberate active restraint, of tendencies to action.* Attention to matters other than the resident kinesthesia of the active arm, as was demonstrated by trial, tends to stop the performance altogether, or to make it irregular, uncertain, and wabbly. Attention to the resident kinesthesia, on the other hand, tends simply to make the movements less, while they continue deliberately to the end without hesitation, but minimized. Considering that the movements studied are free-arm movements from the shoulder-joint, tending therefore to exaggeration far beyond their true size, this result becomes all the more striking. Almost no records, *per contra,* showed enlargement under these circumstances, also a significant fact where the voluntary conditions are so complicated and uncertain. Fifty-five per cent of the reactions showed this inhibitory effect and just about half of the subjects made reactions consistently that were of this type. May we not conclude that the more or less conscious resident kinesthesia, perhaps distinct from other kinesthetic coördinations, has special inhibitory function, as well as the passive reception of jars, vibrations, passive movements, and postures?

The experiments seem to show, moreover, that the muscles themselves are the seat of the changes that the arm undergoes when intensively attended to. The bearing of this fact upon the controversy between Reichardt (60), Pillsbury (57), etc.,

and Goldscheider (32) as to the relative status of the muscles and the joints for kinesthesia, is not easily cleared up at present. It would tend, if anything perhaps, to support the former, the muscle, side, for reinforcing the resident conscious sensations (nerve-impulses) lessens the extents rather than disturbs the shape based on the (extrinsic) motor ideas. This inhibition of a gross complex movement we must ascribe directly to the muscles, whether *guided* by the arthrodial afferent impulses or not.

The fact of the prompt and usually accurate *visualization* of the movement-series received into the person's central nervous system solely through the kinesthetic receptors of the arm, is an interesting product of this experimental work. Often the subject reported a clear visual image of the norm after receiving it (kinesthetically) only once, and often he proved its existence and its accuracy by drawing it (on the basis of a visual motor idea). The paths of these kinesthetic impulses and their production of a visual image constitute a problem of especial interest for the physiology of efficiency; and they would explain the conception of space in the blind. This process of visualizing varied much in different subjects. It was best and promptest in visualizers long trained in delicate motor adjustments. It was least conspicuous in the three subjects (two young women with normal vision and a youth congenitally blind) whom the psychologists would class as motiles (see below).

In any case, this prompt and usually involuntary, translation of a complex and purely kinesthetic impression into a visual image is sufficiently striking in itself, however familiar a process, and leads inevitably to a query, as yet unanswered, —why? Why cannot and does not the man of average motor skill use his conscious kinesthetic impulses in the guidance of a movement, especially a movement about which he knows nothing else? Perhaps one reason for his failure to do this lies in the inherent inhibitory action of the conscious kinesthesia—and the other phase of kinesthesia his imagination knows nothing of. But aside from this in part, the *opposition* between the resident sensations of movement (inhibitory and more or less conscious) and the 'visual' and other actuating influences, is marked; and the fact may prove suggestive. Even when reinforced by conscious effort, the resident kinesthesia and its centers (in the great cortex?) had usually no conscious memory of the norm as a whole, had nothing, in fact, usable as a motor idea; and the subject (unless a motile) thought himself wholly dependent for an efficient performance

on a second-hand, mediate, visual image, product of the immediate impressions none the less which he supposed himself unable to follow as a guide. The apotheosis of 'the queenly sense of vision' is close at hand, and that notwithstanding only a few to-day suppose the eyes have more to do with a bodily movement than the placing of its termini (*a quo* and *ad quem*),the location, in short, of the ends of the action in the essential space. (Besides this, in cases of paralyses, etc., vision serves vicariously for kinesthesia in many ways; but we deal now only with normal conditions.) The conclusion is inevitable from these circumstances that there must be kinesthetic nerve-impressions in closest relation with the visual neurograms proper, yet obviously unconscious, that do furnish the data necessary for a voluntary movement, and that actually do coördinate the muscles which are active in an action. This phase of kinesthesia, actuating, determinant of the force and speed of a movement perhaps, but subconscious or even unconscious, represents, it seems, the innate impulse to activity, the instincts, the emotional syndromes, the reflexes, the correlation of all the forces behind a voluntary arction. This phase of the movement-'sensations,' however, do not represent the individual personality of a human being until complemented, held in leash and inhibited, by the conscious resident sensations. We cannot escape terming these actuations, coördinated perhaps in the posterior grey cord, kinesthetic, for their nature and use are inconceivable apart from the joints, muscles, tendons, bones, and skin,—the chief locations of the kinesthetic receptors. The opposition between these two phases of motor control, actuating and inhibitory (the former including vision and audition), is eminently natural for most people, then; but it is an opposition that is in reality an alliance and capable always (skill) of fusion for efficiency.

The various types of method by which people attend voluntarily to these two things at once in the control of an action is well brought out in the subjective reports if not from the records made by the subjects' arms, which question remains for future measurements perhaps to determine. Most often there was a conspicuous alternation between the visual image and the inhibitory kinesthetic experiences, the visually imagined guide being brought to mind just often enough to furnish current control of the movement's extent and the direction of its component parts. The accuracy of these performers was sufficiently good for ordinary behavior. People of this class constitute the great mass of men and women;

they use their eyes, sometimes, to start a voluntary movement and to stop it mostly, and they use their spinal, unconscious kinesthesia to furnish it force, speed, etc.; but they never become generally conscious of their resident kinesthesia, properly speaking, never acquire the personal valuation proper to new and free, because personally inhibited, deliberate actions, except perhaps in certain sets of movements.

The other type, as indicated by some of the reports of the subjects, all of whom were naïve, are able to *fuse* the visually imagined (but in reality mostly kinesthetic) norm with the conscious resident kinesthesia, using them together for the guidance of a new voluntary movement. The records thus made are more accurate than the others differently controlled. Moreover, the performances of these 'fusers' have an important relative stability, a constancy of form and size, and an independence of agencies which, in others, less endowed with conscious movement-sense, disturb the accuracy of the action. The numerous records show this conclusively, and this indicates much.

The people whom the psychologists class as 'motiles,' then, appear from this research to be those who, for some reason, have their conscious resident kinesthesia hypertrophied, so to say, and apparently at the expense ordinarily of the unconscious movement-neurograms and of the visual control. The subjects who were of this type made poor drawings in these experiments. Their records were distinctly less accurate than those of the 'visualizers' in both extents and regularity, with a tendency to rounded angles and curving lines. In motiles, then, the motor idea tends to adequate presentation in terms of the local conscious kinesthesia; but unless this resident plan of the movement be much more than ordinarily comprehensive, it fails in its supposed adequacy as a motor idea.

My experiments and conversations with many of the most intelligent blind people, young and old, connected with the Perkins Institution (far-famed by its miracles with Laura Bridgman and Helen Kellar) lead me to a complete corroboration of Treves' ideas (69) as somewhat opposed to those of Haller (35) on the elaboration of kinesthetic impressions into the conception of space. We can, however, go two steps further perhaps at present, and point out how similar is this blindman's space to that of seeing persons, and how organic and indispensable it is in both in the evolution, the origination, and the adult practice of voluntary movement. Our records as well as our notes show that the seeing motile is practically in the condition of the congenitally blind, but, of course, far

more accurate in his action and with much greater potentiality of efficiency.

Between these partially opposed types of motor consciousness are many indefinite degrees, apparently, according as a given limb is more or less skilful in a given group of movements, in number of course uncountable. It is, then, one of the inductions of this experimental work that the motor skill of a person in general, and also in particular actions, is more or less proportional to his habit and capability of using the conscious kinesthesia for the current inhibition of actions elsewhere coördinated and actuated. As has been shown already, this actuation comes from (spinal?) kinesthesia in combination with external control, usually either visual or auditory. We physiologists are rapidly learning that all bodily processes and conditions are the algebraic resultants of balanced tendencies, whether nervous, chemical, or mechanical. The neuro-physiology of skill as in part determined by the afferent neurograms of movement, certainly is no exception to this rule. The unconscious and the conscious, the actuating and the inhibitory kinesthesia, surely share and complement each other in motor control. A person's skill, therefore, appears to be a 'function' of his habit of usefully *fusing together* his motor ideas proper and the resident movement-sensations which in him are adequately conscious. Compare Slinger and Horsley's conclusion (68) that "the muscular sense under necessity can, by education, be brought to a point at least one-fourth better than that learnt by a normal seeing individual."

But, again, compare the practically unanimous opinion and practice of instructors in all kinds of motor efficiency (music, instrumental and vocal, manual training, physical education, legerdemain, etc.) that attention to the sensations of movement disturbs the performance and is therefore to be avoided. At least one successful instructor in voice, of my acquaintance (Mr. Willard, late of Harvard University and now of the Sargent Normal School), makes this avoidance of local consciousness the very key-note of his method, substituting therefor an intensified general consciousness of effort.

Reconciliation of these two attitudes, one academically scientific and the other purely empirical, but both obviously true, would seem to lie in what has been learned in these experiments, if indeed skill does consist *in a trained fusion of the extrinsic motor ideas and the intrinsic inhibitory conscious control.*

The experiments furnish a certain limited amount of evi-

dence that the resident kinesthetic impressions, of the right arm at least, are relatively inconspicuous in the naïve child, and become more and more manifest with the evolution of motor consciousness and skill, both in general and in particular.

This fact may have important potentialities for future research in the direction of hastening motor efficiency in a properly physiological way. Experiments in this direction are under way, but are beyond our present range.

In our present unfortunate lack of definite knowledge of the special functions of the various kinesthetic end-organs described by Golgi, Pacini, Vater, Kühne, Von Frey, Kölliker, Ruffini, Bonnet, and others, it would be almost rashness to suggest the most likely use of each. The muscle-spindles have often been termed the probable receptors of active innervation; possibly thereafter act in series the Golgi-Manzoni corpuscles at the junctions of the tendons and the muscles, then the Pacinian corpuscles in the synovia, etc., and finally, when the movement is considerable, the receptors in the skin and subcutaneous tissues. Which of these, or whether all, act both for the inhibitory function and for the actuating process that we have called, for lack of better term, the spinal kinesthesia, remains to be worked out. One might expect the arthrodial nervous impulses (32) to be concerned in the actuating processes, as guide-determinants of the initial direction.

The physiology of the reinforcement of the resident movement-impulses by attention to the arm in action is not a difficult matter nor a complex one. As in all other cases, this process leads to a hypertonia of the muscles and this in turn to an increase in the afferent impulses sent inward by the kinesthetic receptors. This increases greatly the delicacy of the (inhibitory) control, the muscles having far more tone and vigor; and defines the sensations better, making them more conscious, thus it may be allowing them to fuse with the extrinsic motor idea (visual and spinal) and meanwhile to dominate the actual movement as representative of the truly personal (cortical) will.

III. Confirmatory Evidence

Corroboration a-plenty of the results of the present research seems to be at hand from several different and wholly independent directions, especially in regard to the proposition which we have induced from the records, that kinesthesia is two-phased, one phase unconscious and actuating and the other conscious and inhibitory.

In the well-known research of Mott and Sherrington (53),

the cutting of the afferent roots of one side in the ape from the fourth cervical to the fifth thoracic, led them at first to conclude that the 'will' was abolished so far as the arms were concerned by thus withdrawing the peripheral kinesthesia from the central nervous system. Munk's prompt repetition (54) of these experiments, while showing also how vicarious and withal how widespread voluntary control may be, demonstrated that when recovery of function had taken place, the movements were invariably *inexact* and *more extensive* than under normal conditions. *Per contra, sed idem sonans,* our experiments have shown that when the conscious movement-influences are exaggerated instead of abolished, the movements tend to be true to the extrinsic motor ideas but less extensive than normally. In other words, again, the resident kinesthesia is inhibitory. Other evidence for this is often at hand in ataxias, functional or organic, when of peripheral origin, the movements then showing clearly a lack of normal restraint. Moreover, it is not improper to suggest the possibility that the reason that all short movements tend to be made too large lies in the insufficient innervation 'reflexly,' so to say, of these inhibitory influences.

The neurotopographers have already found evidence for two separate kinesthetic pathways in the brain if not in the cord. v. Bechterew (5) without dissent says, "Nach seiner (Lewandowsky's) Auffassung setzt sich jede Bewegung aus zwei Komponenten zusammen, aus einem bewussten und unterbewussten Element." Bastian, too, appears to agree with this, from data purely neurological. Again, Russell and Horsley (61) express one of the results of their work on spatial orientation in this manner: "It is therefore, we submit, perfectly clear that what may properly be termed the mid-axial line and region of the hand and forearm, is as definitely represented in the cerebral pallium as it distinctly is in the spinal cord." "The spinal representation of tactility [a variety of kinesthesia] finds an echo in the arrangement of that function in the sensory cerebral centres." It is not obvious why there should be this double representation unless these two sets of kinesthetic centers have in some way different functions. Those familiar with the recent brief but important symposium (6) on muscular representation in the grey cord and in the great cortex, would perhaps be inclined to believe that the 'individual' skeletal muscles are mapped out in the pallium only for some very special purpose, not discoverable by electric stimulation. That purpose now seems to be inhibition (through the resident kinesthesia).

In a closely allied realm of neurology, facts have already combined to form an essential connecting link in support of the main result of our research that the conscious kinesthesia (and by consequence, voluntary movement) are inherently inhibitory processes, just as all other kinesthesia is actuating in action. Perusal of the consensi of competent opinions published in the splendid volumes of Lewandowsky, v. Bechterew, Cajal, and others, makes it extremely probable that there are two main courses for the carrying of kinesthetic influences into the gery regions of the brain. Grant Van Biervliet's finished work (70) already referred to, and there is no longer need of seeking separate pathways or centers for tactile and movement sensations; and on this basis several discrepancies in neurotopography disappear. Touch is impossible without muscular movement; and the sensation of movement in some of its phases is but an inner touch,—indeed the blind know it by no other name. For example, the path ascribed by Cajal to tactile impressions corresponds as well to the 'somaesthetic' tract of Barker and to the kinesthetic tract of Starr as one need expect in the present relative indefiniteness of these complex matters in neurology. There is sufficient agreement already that an important avenue of ascending nerve influences extends from the kinesthetic receptors through the latero-posterior cord, the lemniscus, etc., with relative directness to the cortex of the great hemispheres. Many circumstances suggest that this route, relatively simple and direct, conveys the conscious inhibitory impulses that we have termed the resident kinesthesia, characteristically voluntary or personal.

There is, however more general agreement as to the topography of the indirect ascending pathway between the afferent grey cord and the cerebral cortex; this passes through Clarke's column, (the cerebellar bundle of Flechsig), the vermis superior, the mysterious Purkinje cells, and the thalmic ganglia (van Gehuchten). Barker suggests that ten or more neurones may be ordinated in this passage! This many-swirled flood of neural currents passing to the cerebrum represents, of course, the hereditary spatial moto-sensory outfit of the individual. For our purpose here and now it stands for that complicated actuating innervation of the voluntary musculature under the control of vision, audition, etc., in combination with the positive, impelling, impulsive kinesthesia centered in the spinal grey. It surely is intimately connected with the whole sympathetic disposition of the personality, with his feelings and emotions, with his whole intellectual memory and experience, and its intimate relationships involve the whole brain and much if not all of the whole nervous system besides. On the

other hand, we have to suppose the more direct, conscious, and inhibitory, tract to convey the occasional, difficult, or wholly new personal *effort* of the individual as a personal will, related with especial closeness to the cortex of the hemispheres. Our every-day, ordinary behavior uses this tract extensively; but skill and novelty employ it intensively as the road to even greater efficiency.

Hollingworth's recent work (41) in the Psychological Laboratory of Columbia University correlates its results so far as kinesthetic matters are concerned with the opinions of Sherrington, Titchener, Angell, Woodworth, etc., but makes the ideas more explicit: "Attempts to find a single anatomical or topographical source for the sensation which serves as a criterion of extent of movement are contradictory and futile. * * * * A movement comes to be recognised as larger than others, not because it produces a more intense sensation, nor because of any geometrical correspondence of internal and external points, but because it has been *learned to be a larger movement*, one that will effect a greater change in an object with which we are dealing." Without reference to the fallacy here similar somewhat to the ancient one asking why we see objects erect when the retinal image is inverted, it is obvious that herein is a definite opinion (derived from long experimental work made in the laboratory of men as cautious as Cattell and Woodworth) which confirms our own experimental result that extent of movement is an unconscious function of the spinal grey in closest association always with brain and cord and especially with the cerebellum. A larger movement has been "learned to be a larger movement" by various experience indeed and the judgment gets no help at all from the conscious kinesthesia in this respect. Sometimes the information comes only with difficulty. Over and over in our experiments, records not a half or a third as large as the norm, whether kinesthetically or visually perceived, were reported by the agent as large enough or even too large. In other words, the local attention-strain of true voluntary movement is not translatable into size or extent. The unconscious actuating aspect of kinesthesia takes care of extent, probably, and in terms made up of energy, speed, habit, momentum, gravitation, and other things far too complex to be described at present. The activity of the conscious phase of kinesthesia exerts an entirely different kind of control over action, a control that is truly voluntary.

Further evidence, more or less direct, of the validity of this point of view may be had from the observation of the development of voluntary control during the first year of life. One

fact which has regularly been overlooked in genetic physiology and psychology is the completeness of the inhibitory apparatus and activities at birth. See for example the writer's observations (14) on the little girl 'L.,' made in 1899. A few months later in the ontogenetic evolution, when deliberate control begins to be obvious, inhibition seems to develop or at least to be apparent somewhat before voluntary actuation (14, p. 195). Guyau recognizes the fundamental duality of the will-apparatus, as the following quotation attests: "La pleine volonté, c'est-à-dire le deploiement total des énergies intérieures, suppose qu' à la représentation de l'acte même qu' on va accomplir s'associe la representation affaiblie de l'acte contraire. Et ainsi, nous arrivons à cette conclusion: Il n' y a pas d'acte pleinement volontaire ou, ce que revient au même, pleinement conscient, qui ne soit accompagné du sentiment de la victoire de certaines tendances intérieures sur d'autres, conséquemment d'une lutte possible *entre* ces tendances, conséquemment enfin d'une lutte possible *contre* ces tendances." Ribot's whole psycho-physiology is permeated with this functional duality in action, as all the world knows. The evidence, experimentally produced, that this two-phasedness of the will extends throughout the action-system in a literal way and even to the kinesthesia motivating or actuating and controlling muscular coördination, adds only another factor to our knowledge of how we act.

Weir Mitchell, forty years ago, published (51) material (derived in part from the soldiers of the great War) which throws some light on the neurology of kinesthesia, especially in its relation to actual consciousness. From this, one of the most extensive investigations of the sensations of lost limbs ever made, he produces many most interesting inductions. These statements following directly bear more or less on our research-results, and, in a way, complete them. "Involuntary movements of the absent toes or fingers are frequent and in very many persons are unfailing precursors of an east wind. [*Cf.* with an inflamed joint's ability to foretell the approach of a storm.] Sometimes only one finger is thus active, or the digits flex one after another, and then slowly extend, whilst wrist movements are exceptionally rare, and the elbow and knee are never felt to change place at all.[2]

[2] It certainly is suggestive that the fingers, the most conscious and best controlled parts of the body (save the eyes and tongue) should be those most persistently and most frequently felt under these conditions. The fact suggests more of conscious inhibition in them than elsewhere in the body.

"Leaving out of the question cases which have no sense of the presence of a lost limb, we find that in a very small number there is no consciousness of power to stir any part of the absent members by force of will. All others are able to will a movement and apparently to themselves to execute it more or less effectively, although in most of the amputated such phantom motions are confined to the fingers or toes, which rarely seem to possess the normal range either of flexion or extension. Yet the certainty with which these patients describe the limitations of motions, and their confidence as to the place assumed by the parts moved, are truly remarkable; while these restricted movements are pretty surely painful, and the effort is apt to excite twitching in the stump. A small number have entire and painless freedom of motion as regards all parts of the hand. 'My hand is now open, or it is shut,' they say. 'I touch the thumb with the little finger.' 'The hand is now in the writing position,' etc. Between these cases and such as are conscious of an immobile member, every grade of difference as to motion is to be found, with equally wide varieties as to the associated pain which perhaps is most acute in such as will with vigor a motion that they seem to fail of executing.[3]

"The volition to move certain parts, continues Weir Mitchell, is accompanied by a mental condition which represents to the consciousness the amount of motion, its force and ideas of the change of place in the parts so willed to move. The physiology of the day accepts the belief that all of our accurate notions as to the amount of power put forth and as to the parts thus stirred reach the sensorium from the muscles acted on and the parts moved. It would appear, however, from the statements here made as if coevally with

[3] Here we see restrictive kinesthesia, by reason of some unknown abnormality in the spinal or other centers, so intense as to be painful—just as all sensations can pass over into some experience at least very unpleasant. If we compare the pain here produced with that made in a real anchylosed joint by a strong attempt to move it, we shall find great similarity. In the latter case the pain is obviously due to the overstimulation of the arthrodial receptors, and we may not improperly suppose that the former is due to some sort of an overaction (exerted from the 'reservoir' above?) upon the actuation-centers of the brain-stem, compelling a painful overstimulation of the inhibitory mechanism, but central rather than peripheral in origin in the real joint. In such places and cases as this it is easy to satisfy the craving of those who deny absolutely all that they personally happen not to believe, by over-hypothesizing but let us refrain; the very complexity of the conditions referred to serves as a sort of argument for our present contention of a kinesthetic duality.

the willing of a motion there came to the consciousness, perhaps from the spinal ganglia acted upon, some information as to these points. If, in reply to this, I be told that the constancy of long habit may have associated memorially with certain ganglionic activities the ideas of local movements, I should hardly feel that this was an answer, because in some of my cases the amputations took place so early in life that there was no remembrance of the lost limb, and yet twenty years after, a volition directed to the hand seemed to cause movement which appeared to be as capable of definite regulation, and was as plainly felt to occur as if it had been the other arm which was moved. Probably, then, a part of those ideas which we are presumed to obtain through the muscular sense are really coincident with, and necessitated by, the originative act of will or else are messages sent to the sensorium from the spinal ganglia which every act of motor volition excites."

As a monistic interactionist, so to say, the writer is willing to accept either of the alternatives suggested at the end of the above quotation; but to him as an out-and-out physiologist (as ignorant as a biologist of anything like metaphysics), the latter of the two seems the better. Especially does it seem the better because it undoubtedly adds an important item to the neurology of kinesthesia: namely, the needlessness of supposing that literally all kinesthetic impressions come directly and separately from the joints, muscles, tendons, bones, viscera, and skin of the moving parts. We may properly suppose, on this timely cue, that *the spinal grey is so intimately aware of all that goes on in the action-systems* (through tides of intelligence unifying all parts of the circuit) *that not only actuation but (inhibitory, conscious) control can arise and be exerted there from further up, from the great cortex.* In other words, here is direct evidence, hard to refute, that the cortical grey acts by and on *movement-complexes* represented not only in the cortex but in the grey matter of the afferent cord, the musculo-(et al.)spinal circuit being an unity in the most literal and absolute sense. This is precisely in line with the tendency in interpretations of the action of the nervous system—indefinite conscious and subconscious grouping and subordination and "symbodism" beyond present detailing (see below).

Finally, for the present discussion, Sherrington's classic work (64), especially that part of it relating to the reciprocal innervation of muscles that are functional antagonists, doubtless has direct bearing on this corresponding division of the afferent movement-information into inhibitory and actuating. Nikolaïdes and Dontas's demonstration (55) of inhibitory

neurites in the frog's motor nerves (confirmed by Wooley and Fröhlich) is also suggestive of further knowledge to come. In general, however, the inhibition of a voluntary movement in current control through its conscious resident kinesthesia relates more to molar inhibition by groups of antagonistic muscles than to inhibition within the influence of a single neurone. One group of muscle-units actuates a certain movement and the movement is usually useful to the agent only when balanced and restrained by another group under conscious kinesthetic control.

Aside from the fundamental discoveries by Sherrington (64) already referred to, Lee and Everinham (48), Camis (10), and Forbes (29) have recently shown that in some mode or other separate kinesthetic paths or "nerves" control separate groups of effector neurones which in turn supply respective muscle-groups, so that we know now that there is far more division of labor among at least the afferent neurones than was suspected formerly, so far as their control of muscle is concerned in particular. Our research seems to go one step still further in this differentiation, suggesting, as it does, additional subdivision of the receptor pathways into actuator and inhibitor as already set forth. This seems to complete the logical requirements of mechanism for the understanding of the actual behavior, complex far beyond actual description at the present time.

IV. The Impulse to Activity

Just as the essence of life considered for the purpose as a 'material' process is movement, movement universal in time, in place, and in meaning, so truly is the essence of the function of the nervous system to coördinate these movements, whether molar or molecular, whether wholly within the body or the body as a whole with its particular environment. The very meaning of protoplasm physically speaking, is motion. This universal inherent necessity of movement in organism is known in psychology and in educational theory as the impulse to activity (and all too late has its universal necessity been recognised!). Sometimes a fully conscious thrust, sometimes pushing quite as hard but all unconsciously, this all-pervading vital impulse is the motive power of all achievement. It represents *actuation* of both the vegetative and the personal in human efficiency, just as the conscious personal will stands for restraint. The greatness of its importance in education and in explanatory psychology we need not pause to more than mention.

Through many thousands of varied receptors the ever-active

tissues start and send a flood of impulses inward to the central nervous system, (the sympathetic partaking in this primal function); and this flood stops only with the abolition of protoplasmic, especially muscular, tonus, at death. This obviously is the 'spinal' kinesthesia described to some extent above, fused intimately so far as service goes with the visual, auditory, and other receptor impressions, especially the first. This flow from numberless motile springs keeps full the reservoir of kinetic or potential energy on which the whole effector side of the organism's activity continually draws for its life so far as coördination and guidance are concerned. These obviously underlie and determine the entire motivity, both vegetative and personal, of man.

Complex beyond conception is this basal kinetic impulse, psychologically as well as neurally; and we shall not, nor need we, make any attempt to describe either its phenomena, or its mechanism in anatomical terms. It stands for the motivation of behavior and its apparatus therefore must include every active factor of the psychophysical organism that makes for mere activity, for unguided, unvoluntary movement. Conspicuous in this ultimate or nearly ultimate integration is, of course, the whole conscious and subconscious process of emotion, and the two basally opposed feeling-tones of euphoria and dysphoria so essential in efficiency as well as in human happiness. In closest relationship, too, with this, the vegetative back-bone, so to call it, of the organism, are all the sensation-'centers' (spinal possibly and surely thalmic) and all the psychomotor 'centers' concerned in the motor adaptations of each of them. In short, the whole nervous system more or less is at once affected by and affects the bodily mechanism of the impulse to activity, behavior's motivation-tracts.

Behavior, indeed, physiologically viewed, is controlled motivity and the control is the circuitous guidance of the reflex and voluntary kinesthesia. Thirty-five years ago (a long time in the history of the feelings of movement), Stanley Hall, in a neglected article in *Mind* (34), expressed the basility of its nature thus: "The feeling of motion we have shown is the simplest, earliest, most universal, known, psychic rudiment of animal life. It is distinguished from every other sensation in being identical with its objective cause or aspect which is also motion." This uniqueness of kinesthesia has been too little appreciated in psycho-physiology, and its significance hardly understood in its metaphysical relationships as well as in its physiological bearings. The impulsive phases of the motion we should now think of as subconscious, the sensation

as conscious, representing intensive control, coming rather from the voluntary personal inhibitory guidance, as already set forth.

Let us now look for a little at a rare condition known in its typical form to nerve-specialists as *abulia*, for by so doing we shall accomplish two things at once, more or less completely: we shall illustrate the absence or negation of the else universal impulse to activity, and perhaps help to explain this interesting condition.

The term abulia is Greek for will-lessness, absence of volition proper; the research and literature of the subject is mostly French. Rare or perhaps unknown in childhood (in itself an interesting theoretic circumstance), abulia is found oftener in women than in men, and consists simply (so far as external phenomena are concerned, it is simple enough) in a functional inactivity, a will-lessness carried to any extent, it thus being sometimes a matter of life and death. There is no loss of muscular power and no disturbance either of the sympathetic vegetative musculature or of the reflex habitual behavior, the abulia relating solely to the deliberate motivity. Understanding in simple abulia is unimpaired, so that the patient may have the most vivid concepts of the necessity of action. Actual cases are often complicated, of course, with many phases of hysteric, neurasthenic, "psychasthenic," and psychiatrical conditions, but pure abulia is inherently a lack of deliberate conation.

One of the natural results of the obvious exhausting 'overworking' of the Rolandic area as the recondite 'center of voluntary movement,' is an awakening of interest in this condition. Logically speaking, it is the only known defect in voluntary action proper, for it apparently is due neither to a true paralysis of nerve-knots nor to disturbance in muscles or their nerves. Abulia is of especial interest on this account.

The abulic's ideas that she should or must act are unexcelled examples of mere ideas, associative products that are of service to no one and to no thing—the grist of intellectual mill-wheels that can only be thrown away when it is ground. The very notion of ideas unexpressed in any way, unmixed with feeling-tone, unmaterialized by will, has in it something ironical, something artificial and unreal as if the by-products of the language-processes characteristically human to be sure, but none the less with inherent liability to useless action—like some neglected grist-mill by the brookside whose stopping-mechanism has been deranged, and which, until worn out, makes noise and dust and jolt and jar beyond all reason. In

some such mode appear to the writer to be any association of ideas never used because never materialized in real bodily action and thus related to the rest of Nature. Such, in a sense, are the psychologically interesting ideas of the abulic that she should act, must act,—and yet does not. Mourre, for example, considers abulia to be inherently the inability to transform an idea into its act as the product or consequence of victory among antagonistic ideas. The abulic, he says, knows that she wishes to do a thing and at the same time knows that she does not do it, because the inhibitory association of ideas prevents. In true abulia, he thinks, the patient deems her will free; but she fails to act because she chooses not to do so, fearing she might fail. There may be also, he supposes, a diminution of the vital sensations and desires, making the motogenic balance of pleasantness unusually small. The physiology of abulia, Mourre concludes, is some organic neural disturbance, while the psychology is the difficulty of voluntary effort.

Sometimes abulia follows an asthenic infectious disease, *e. g.*, typhoid, as in a case described (59) by Raymond and Janet.

There is no other condition quite like abulia. Melancholia, katatonia, certain phases of paranoia, and even some cases of dementia and of amentia, exhibit somewhat similar external bodily conditions (that is of deliberate inaction), but in all of these the conscious motivity is wholly different in ways both too familiar and too complex to allow of present rehearsal. Here in abulia alone have we a defect in the very physiological *motives* of the personal activity.

One is reminded inevitably of the numerous persons who, it is customary to say more or less sarcastically, were 'born tired,' who are in short, to the uninformed lay-mind, lazy. The world still has in mind the dire hook-worm's timely vindication of his thousands of victims in the South, and now perhaps it is another (but relatively small?) group of inactives and inefficients, the mild abulics, to whom we owe the courtesy of excuse. The king of optimists himself would not wish to deny that there are plenty of individuals of many sorts whom habit and inclination have made truly lazy; but certainly there are many others whose tendency to inaction must be classed as a mild degree of abulia, those especially with a psychasthenic *Belastung* whether constitutional or acute. In thinking over the persons of this type in one's acquaintance, one finds, in surprisingly large per cent, evidence of deranged metabolism of the depressive trend, a low metabolic plane of

efficiency with the balance leaning toward mal-nutrition. Physiologically these individuals oftentimes are cases of functional abulia of a mild type, but more complex and so harder to unravel than even the well-pronounced cases with the voluntary mechanism obviously deranged.

The one indisputable argument for defense in these conditions bases itself in the fact that to a thoroughly normal person, activity is pleasant. To the sound mind in a truly sound body a normal amount of suitable work is a delight the most reliable if not the most intense, of all human delights, whether it come from the deep action of the diaphragm or from the normal routine of the day's vocation, whether 'sport' or 'work.' Three conditions may be noted in passing, related to this matter that are among the hardest of the great world's cruel facts and which have gone far to prejudice mankind against the idea of work: *1.* multitudes are engaged in ill-adapted and hence uncongenial work; *2.* multitudes, including these former more or less, are working longer if not harder every day than a later age of economics will allow; *3.* multitudes, more even than the others, are not in that normal neuro-musculo-visceral vigor to which even proper work is a gratification. Had the world as yet ameliorated out of these harsh primeval conditions of a livelihood, one of the well-based prejudices of our race, that work is hateful and to be avoided, would already be of only historic anthropologic interest like the preglacial distribution of the hairy Ainu. All this to introduce the negative premise that psychomotor activity under perfectly normal conditions of mutual adaptation between the individual and his work renders a distinct balance of pleasure to the maker—yet work, unlike virtue (Twain), is not its only reward. Perhaps it is unnecessary to point out that exertion renders its inherent recompense largely through the exercise of the sense of kinesthesia—the music of the feelings of movement, tribute of the rhythm of katabolic expending, the great joy of motion, index of our life.

We may properly go a step farther without fear of serious contradiction, and say that when this euphoric balance is lacking, bodily activity in general tends to be reduced to a minimum; on the other hand, when the truly metabolic bodily activity is lacking (tonus being the true index of this activity) the euphoric balance is zero or actually dysphoric. Those of my readers who have suffered, for example, from a long exhausting illness will readily appreciate at first hand this state of atony in soul and body—a will-less condition of carelessness, passivity, a *laissez faire*, not of necessity, perhaps

seldom, painful, but wholly negative so far as deliberate action is concerned, and often so to a very surprising extent. Matters which a few days or weeks before seemed of the utmost personal and family importance are now, in this functional abulia (so to call it) completely indifferent, whether they be done or not—the fortunate condition which very frequently indeed, normally in the aged, deprives death from disease of all unpleasantness to the chief actor in the common tragedy. The main factor in physiology of this condition of illness is certainly an akinesthesia arising in the decadent atony of the body and soul. When one's foot "goes to sleep" from pressure on the nerve there is a condition oftentimes almost purely akinesthetic and one interesting for the ready observation of voluntary movement lacking in the local or resident kinesthetic influences. Compare it with the exalted consciousness (agony) of the opposite state as exemplified in hydrophobia and in strychnine poisoning and its tenor is still more clear. In nightmares, too, sometimes there seems to be a more or less general tonic spasm of the muscles so that the sufferer is unable to move against the opposing terror, to jump out of the way, to cry out, or what not. Perhaps in these cases the painfulness of the experience is due to over-stimulation of the conscious inhibitory kinesthesia, for some reason for the moment not under voluntary control. By force of will made up in the waking state and impressed on the subconscious, this cramp in the nightmare may readily come under habitual voluntary control and its pain thus be avoided. (Personal communication from John Frederique Herbin.)

The writer has already in various places (especially 15) set forth, in a manner more or less orderly, his particular ideas as to the neuro-metabolic basis of the fundamental emotional tone of euphoria or of malaise. The primal sympathetic centers are concerned and without much present doubt (38) the optic thalmus, both being, of course, always in the closest possible and most intimate relation with the remainder of the cerebral and spinal grey. All these grey nuclei of the vegetative life (representing as coördinators or adjustors the moods temperament, dispositions, organic habits of every phase, degree, and permanency) are probably under the unceasing influence of those afferent neural influences which we have called the actuating kinesthesia. The reservoir of nerve-center energy, thus largely supplied with tone and information, furnishes at once the nervous motivity and the conscious incentive, which, because at least mildly pleasant, normally urges to activity. When lacking this pleasant tone or in such a condition possibly

of 'block' that knowledge of this pleasantness cannot come into the brain (save *e. g.,* by way of suggestion or hypnosis?), there is the functional condition of abulia, or so it seems likely.

To our present understanding, then, the abulic state, whether thus 'functional' or organic, whether primary or secondary, is a defect or a default in the impulse to activity. It is represented inherently, we may neurologically conjecture, by some disturbance in the actuation of the voluntary muscles probably as much central as peripheral. In abulia, obviously, the muscular tonus is much lowered and this greatly lessens the kinesthetic influences. On the other hand, the spinal afferent grey also has lost much of its tone, partly because of local mal-nutrition perhaps (a decrease of the chromatin?) (25), but partly too probably because its normal stimuli or supplies of exciting energy from the muscles are partly cut off in part too doubtless by some unknown derangement of the central adjustor-mechanism. This intimate inter-dependence of the musculatures, especially the voluntary, and the will need not be detailed here. The kind and degree of will that would be possessed by one 'completely' paralyzed is too esoteric for scientific discussion; meanwhile no case of truly complete paralysis (that is, an instance in which the central innervations, afferent and efferent, of the muscles, the essential thing, are abolished) has been described, for a person in this condition would be practically dead.

Another condition involving deficiency in the common impulse to activity and probably in the general kinesthesia fundamentally, is *feeblemindedness,* now so prominently before the educational and economic world. I am not familiar with any definite information or even suggestions as to the etiology of this important condition, the ordinary theory being simply that it is related to some arrest or mal-development of the cerebral neurones. Granting that this is likely, it is interesting to look further at the implications of this defect.

We have already sufficiently pointed out the flood of nervous influences that is continually pouring into the nerve-axis, chiefly from the action-system, the skin and the viscera; and we have indicated that these influences become integrated, in the higher levels of the cerebral grey, into conscious (largely inhibitory) motor ideas or into emotions or emotional tones which act as resonators and so produce the effect, the '*animus,*' behind and beneath all voluntary behavior. Only a brief consideration is needed to convince one how important these streams of kinesthetic and coenesthetic impulses must be in the mental and bodily deficiencies of feeblemindedness.

Whether central or peripheral (more likely it is both), the afferent feeders of the psycho-motor reservoir are probably determining factors in this condition.

It has scarcely been sufficiently emphasized as yet by competent writers on feeblemindedness how frankly motor is the basis of this defect, phylogenically or ontogenically,—the latter, the personal, factor playing perhaps the leading rôle. This factor is getting more and more of its due importance. For example, in the very latest discussion of the brain of a very young human foetus, the authors (7) say: "There is very definite evidence that, in the complex of phylogenetic and ontogenetic factors which subserve the process of evolution, the latter from a very early period play a predominant part."

Norsworthy (56) finds no difference in kind between the feeble-minded and the normal and concludes that "in general, at least so far as intelligence is concerned, idiots do not form a separate species, but simply occupy a position at the extreme of some large distribution, probably approximately that expressed by the normal probability curve." But, of course, idiots are not especially efficient members of society, and an underlying defect very general in nature must be sought in some phase of the moto-sensory organism. Shuttleworth and Potts (67) certainly suggest a forward step in their remarks on the sense of 'touch' (kinesthesia) in the feeble-in-mind: "The tactile function is not only the most general, but in some respects the most important of our senses, and in the normal baby its evolution takes precedence of the rest. Impressions through the eye and ear are criticized through the sense of touch, and this natural development, so serviceable in the spontaneous education of all healthy young animals, [cherche le chaton!] must be imitated in our endeavours to bring up towards the normal standard the sensorial training of imperfect children. In some cases we shall find coarse, insensitive hands which must be drilled into sensibility by grasping hard and soft objects and discriminating the resistance of and the surface impressions of such varying substances as polished marble, sandpaper, velvet, silk, etc. * * * * Such lessons will, of course, form incidents of the object-lessons which play so important a part in early education."

Unless the present writer is misjudging, there is here suggested the defect which above all others is fundamental in the etiology of feeblemindedness. The statistics recently published by Goddard of the Vineland Training School (31), from measurements made in nineteen institutions by as many observers, will be of great importance in the general theory of

the relations of body and mind. These show a correlation between birth-weight and feeblemindedness (the feeble-in-mind being heavier), and between growth and feeblemindedness, the growth of the defectives being much more erratic, earlier to stop, and less in the total than is the case with normal children and adolescents. Growth, it must be recalled, is a reaction to stimuli; and the stimuli in defectives are probably deficient in some way. Here is what one might term a structural ataxia wholly homologous to the functional ataxia which is the one always obvious element in the behavior of these inefficients (aside from the moral "degenerates").

G. E. Johnson (43) indicates that in the feeble-minded the spontaneous movements, which are, of course, the most conspicuous phenomena of this class of defectives, are "of the fundamental kind and not accessory," meaning thereby that they are confined to the gross muscle-movements, primary or elementary, in all our animal life rather than to the delicate adjusting movements of personal control. Swaying of the trunk, motion of the jaw, swinging of the arm, rolling of the head, or the simplest finger-movements are among the most prominent of these fundamental movements. Moreover, says Johnson, "in the willed movements the difference between the control of the fundamental and of the accessory muscles was much more marked in the feeble-minded than in normal children. This was the more noticeable the greater the degree of idiocy. Some who could execute gross movements with regularity and control, were wholly deficient in the execution of finer movements. Even those who walked strongly were utterly devoid of the grace which results from a well-developed sense of muscular coördination and control. Nothing is more striking than the clumsy awkwardness of idiots. Sometimes where the control of the fundamental had been nearly perfected, there seemed a positive gap, as if the accessory had not developed." The proportion of finger-movements to arm-swings in the feeble-minded to those in the normal boys of like age (13.6 yrs.) was as 81 to 93.6. Here certainly is evidence that a deficiency in the conscious kinesthesia if not also in the unconscious actuating kinesthesia, is probably a factor in feeblemindedness whether the lack be central or peripheral or both.

Down (26) says: "Want of muscular coördination is the great fault of the feeble-minded."

Goddard in a personal communication to the writer says: "We have no data at all on the question of blind-folded subjects except incidentally; some of our visiting psychologists

and others have tried to do [the form-board test] blindfolded and as I remember it, their time was often as much as three minutes. I should expect that the majority of the feeble-minded would never get the blocks in place." The form-board as most of my readers doubtless know already was an invention of Seguin and as adapted by Goddard consists of a board "of wood an inch and a half thick, sixteen inches wide and twenty-two inches long, having in its upper surface ten more or less variously shaped depressions into which are loosely fitted wooden blocks of corresponding shapes [but thicker],—a circle, a square, a rectangle, triangle, star, diamond, hexagon, half-circle, and oval. The test consists in placing the blocks in their proper depressions, and the method, accuracy, and speed are all factors in the ensuing judgment of the child's mentality" (A. Holmes: 42).

Without multiplying such evidence of the aimless and ill-directed gross motivity of the feeble-minded resolved into akinesthesia, we may reasonable express the tentative opinion[4] that perhaps even the essence of feeblemindedness is some lack in the kinesthetic receptive fields or centers. These influences or stimuli continually bombard the central adjustors and so give rise to the activity which, guided and helped by the cerebral grey (biological will and intelligence and motive), results in curiosity, interest, psychophysiological evolution and education. Education is the reaction of personality to its environment; and reaction is inconceivable without kinesthesia. Certain is it that I at least can get no clear concept or understanding of the vegetative impule to activity in a complex organism, which lacks that kinesthetic deluge of energy into the reflex motor centers by which directly the voluntary muscles (whether also under vegetative dominion, or not) are actuated. In short, the 'attitude,' actuating and inhibitory, unconscious and conscious, is the creation, to a greatly predominant extent, of kinesthesia.

V. Conscious Control

Corresponding as homologue to the actuating sub-conscious or unconscious vegetative impulse to activity is personal conscious control of this activity and of the tendency thereto. How large a part the resident movement-sensations play in voluntary control of body and mind is little realized by the great world of practical educators (manual-training instructors, gymnastic teachers, school-teachers, etc.), not less than

[4] This indeed should not be hard to prove or disprove histologically by one fitted and inclined to the important research.

by the world's other army of industrial workers and employers. Some of the thoughtful psychologists,—as may be seen from the year's and last year's researches,—are awake to the pressing need of application here, application whose reward will surprise many by its size. Here lies the road which he who reads may run. That the road, however, is none too well known, even to extensive travellers along biologic highways, is illustrated in Professor Lehmann's extensive new "Grundzüge der Psychophysiologie," wherein, out of 742 pages, kinesthesia occupies less than three! But with this we are at present not further concerned. Kinesthesia is coming into its own theory, as it is sure to come into its own practice. See, for a recent example, Ioteyko and Kipiani's work (42 A and 42 B) on the ease with which children are taught kinesthetically to write and to draw; compare Montessori's general method (51 A).

H. C. Warren, for example (72), promptly recognised it as the reason for the extraordinary success (whether advisable or not!) of Doctor Maria Montessori in teaching the little children of five to write: "On account of the careful preliminary training in the motor equivalents, they form letters more accurately than the ordinary child who is merely attempting to imitate a visual copy. The words which I actually saw written by children of five and six were far more regularly formed than those of most ten or twelve-year-olds taught in the ordinary way. Moreover, the act of writing really interests the child, and he is constantly practicing it and improving his chirography." In inculcation and habit of 'motor equivalents,' underlain always by intensive kinesthetic attention, lies the undoubted short-cut to motor skill, as the writer has been preaching in a desultory way for some years. At present our concern is briefly with kinesthesia as related to the personal will proper.

It would be fatuous to rehearse the important considerations, however interesting when new, by which we have learned to regard the highly civilized and cultured adult as a product of personal (or of inherited) inhibitions or restraints of instinctive and reflex tendencies common to the more complex brutes and to savage man. Yet, few theoretical things are of more importance than this if we would truly understand the human personality. Man is man indeed only because racially and personally he has grown into the habit of inhibiting himself from brutishness or at least from savagery. Sociologically and biologically the essence of manhood is restraint,—inhibition of his impulses to untoward activity. Vegetative behavior is inherently impulsive, reflexly conative, but a *man* is char-

acteristically *an inhibitory agent with much,* complex and various, *to inhibit.* That is, in brief, the human impulse to activity is a most complicated algebraic correlation of forces or tendencies, full of new conditions to be met continually, and demanding, therefore, ever new combinations of the sensation-influences which orient him in his material and spiritual environment, and which constitute him, and thereby constitute him, man.

In general, then, conscious control, being a matter for the greater part of the cross-striated musculature, is apparently dependent on the conscious inhibitory kinesthesia, since these nerve-impulses, as we have seen, are especially related to the careful, that is, personal, adaptation to needs in skill, in grace, in every kind of efficiency dependent on adjustment as distinct from actuation. Less inadequate discussion of this phase of the feelings of movement is reserved for another paper when further experimental evidence of its importance can be offered.

VI. Notes on the Neurology of Meaning

From a largely physiological viewpoint we have, in the foregoing, described much that properly may be said to be the basis of the will. The will, however, is more than body in action, more even than the neurocircuits which control its action. Will has mental aspects as well as somatic, relations of human personality outside of matter, *meaning* as well as motion. In the results of the preceding research-work, particularly in the duality of kinesthesia's phases, there is obviously the suggestion of a contribution to the mental aspect of volition and, indirectly at least, to the euphoria-dysphoric basis of feeling.

Part of our mind's meanings are firmly related to the vegetative mechanism, hereditary and mostly subconscious, which underlies the more properly personal elements of our behavior. On the other hand, many meanings, equally efficient or more so as stimuli to action, are characteristic of the personal freer side of our common nature and may have only indirect relation to that part of ourselves that defends us, feeds us, and leads us to the getting of children. Perhaps it will seem not improper if we quote from a previous article on apraxia (18) the following hasty outline of a typically vegetative meaning, namely the meaning of space. This is an all-important meaning for humanity, as Immanuel Kant long ago expounded and many, notably Lotze, since. So far as behavior is concerned, space is, of course, one of the substantial frameworks of our experience.

As the writer has elsewhere (*Moto-Sensory Development*, 1910) in a general way suggested, observation of the development of voluntary movement in the infant during the first three years or so (a much-neglected medical field) indicates that power of deliberate action, save in the simplest innate movements of reaching and clasping with the fingers, develops *pari passu* with the conceptualization of space. This, we may not doubt, is much more than a coincidence. The infant is at first an all but reflex thing, moving under the stimulus of the inherent impulse to activity, and he becomes a free agent, personality, only as he becomes aware of the empirical three dimensions in which his body, like his wholly objective world, is constructed, and in terms of which alone initiative movement acquires meaning in any logical sense. In these terms alone to the finite mind is orientation or order: can any one, then, suppose that this universal ground of action is unrepresented in the integration of his will? * * * *

So far as spatiality is concerned (surely in its relation to deliberate movement a case in point) the "reservoir" in the young infant's brain fills from many afferent sources indeed, and they, like other complex neural conditions, are beyond present exact detailing. Because he could not draw them wholly on the map, no one doubts that New England is covered with a network of railway tracks. Among the factors of this working knowledge of spatiality, however, in the evolving infant mind and therefore mechanized in the adult, are certainly these: *A*, the resident kinesthetic influences from the limbs moving so continually under the impulse to activity, and especially those coming from the joints, muscles, tendons, skin and bones of the arms and hands, pioneers in the evocation of the deliberate will. *B*, the kinesthesia from the extrinsic eye-muscles and from such other important and widespread muscles (of the neck, back, abdomen and even legs) as assist in directing the line of sight always on the (stereoscopically placed) foveæ centrales. *C*, the kinesthetic and special impressions integrating into equilibrium and coming from the semicircular canals, the eyes, and perhaps from the protopathic receptors of the abdominal viscera. *D*, in some way as yet hard to understand, the local signs of the surface and perhaps somewhat of the interior of the body, especially of the mouth cavity. *E*, the motor mechanisms of each of the senses so far as involving adaptive or adjustive muscular movements that afford awareness of direction or of space directly, for example the tongue, the retina being a conspicuous other example on the olden-time natavistic theory of spatiality. All these and

probably other afferent influences gradually teach the baby that there is a *depth* for his deliberate exploration. A little later the kinesthesia from the locomotor mechanism, that of creeping, tumbling, walking, etc., enlarge and vigorously confirm the spatial concept thus intrinsically developed. Into the details of these various factors of original space-conception we need not even attempt to go, nor is it necessary to do so in order to be assured that their afferent impulses come from every portion of the body and directly or "symbolically" involve every portion of the brain. One need only consider how widespread is the visual "tract" through the hemisphere 'fore and 'aft; how expanded and how involved are the auditory centers if we include the essential vestibular functions; how spread-out and interknit are the taste "centers," those of smell, of touch (shown by Van Biervliet recently to be a phase of kinesthesia) and of orientation, equilibrium and local signs. Such in the sketchiest of outline only is something of the brain-neurology, it may be, of the afferent intelligence that fuses into the fast-growing infant mind as a basal working knowledge of the space in which alone he can move and so have his being.

In proportion as the individual intimately knows this space as represented to him thus by the nervous system is he capable and skillful and efficient, master of himself and of his plastic environment. In short, in the neural (and muscular) mechanism that develops originally a personality's notion of space we appear to have the foundation more or less of the neuromuscular mechanism of voluntary movement in its most obvious details and framework both at once, and what develops in the infant becomes the (often subconscious) determinant of the behavior of the adult.

Let this be taken only as an example of a whole group of meanings in which body and mind exhibit, both subjectively and objectively, their inherent unification in some transcendental mode that we cannot understand.

Obviously kinesthesis is the chief immediate means to our percept and our concept of spatiality.

Another whole important range of meanings, namely those of the emotional sort, have their ground more or less frankly in the kinesthetic and coenesthetic influences (the latter being largely kinesthetic). Exposition of these meanings would furnish material for interesting monographs in plenty; and few topics in the relation of body and mind would render more profit of practical scientific and educational use. Mind has a motor basis, and nowhere else is the direct connection of

kinesthesia (actuating and inhibitory) and significance more clearly to be seen than in the affects. Graphic and plastic artistry amply show this motor ground, as all who are familiar with physiological esthetics clearly recognise. It is music, however, the freest of them all, in which the kinesthetic factor is most apparent. Of course, the over-evolved music of the ultra-'classic' kind, more intellectualism than feeling, exhibits this basis less clearly than does real music, the purely affective musical music of the opera, the ball-room, and the battle-field, intended for enjoyment or for use rather than for a bastard instruction in artificial technique. In the experience of this real kind of music, conscious kinesthesia is altogether dominant, and provides the psychological framework of this stirring mode of experience. This domination of the rhythm-aspect of music (and rhythm, I take it, is wholly and inherently kinesthetic and coenesthetic) has generally been recognised, but in my opinion always inadequately, as, for example, even by the latest writer on the subject, Weld (73). Lack of space makes inexpedient the elaboration of this important relation here. In general, it is to be observed that the vibrations that form important parts of many kinds of experience, and which are perceived, of course, wholly kinestically, have so far failed to receive the attention they deserve at the hands of psychophysiologists. Helen Kellar (44) has some extremely pertinent and interesting remarks anent this theme; and what to her is clear consciousness we may be sure is at least subconsciously but none the less *effective* in every one of us.

How unifiedly intimate the receptor impulses are in such examples as we have suggested on the one hand to bodily movement and on the other, logically quite different, to our consciousness, is apparent almost at once. Without kinesthesia, space is a meaning clearly unintelligible; and music based on rhythm is well-nigh inconceivable. Wundt's principle of 'creative resultants' has long been the model for explanations of meanings similar to these. They who refuse to admit the efficiency of this method[5] must be surviving believers in that narrow notion of mind that limits it to immediate awareness, ignoring the vast complex deeps of 'the subconscious.' Numberless meanings have been synthesized out of the primary sense-experiences (attitudes) in every normal child's brain and mind and the neurility representing them in the adult has long since sunken out of consciousness, save as intensive (more or less inhibitory) effort in skill or other new and personal

[5] For instance, Wm. McDougall (50) in his brief clear argument, "The Psycho-physics of 'Meaning.'"

deliberate effort floats them to and on the conscious surface of the stream of mental action.

Just as every intended voluntary movement depends for its integration and realization on the implicit spatial frame-work of kinesthetic, visual, auditory, etc., neurility, kinetic or potential in the central nervous system, so every bit of understanding that helps constitute our minds or serve as a basis of volition, has its basis, mediately or immediately, in the two-phased innervation of the vegetative and the personal will. The far-reaching practical value of this understanding, of these meanings, may be exemplified in the use of objects (eupraxia), material or otherwise. Each object, in so far as deliberately made by sane man, has a meaning, namely, of course, its final purpose or use. This use, in general, is not understood as a real understanding save as the individual actually uses the object. Of course, this information may be and often is obtained at second-hand, as one learns from books instead of by first-hand observation; but we have to suppose the brain-paths concerned to be the same substantially in both cases—namely, the grey fabric of the hemispheres in its entirety or in part.

The *early elaboration of human behavior into very numerous psychomotor complexes* on a basis of economy and habit devolves especially, we may reasonably presume, on two important portions of the brain (connected intimately, however with all the rest)—the frontal lobes (via the central) and the cerebellum. If we know anything at all of the frontal cortex it is that one, at least, of its duties is this elaboration of motor ideas, how to use objects, for example, or how to rotate the arm or roll a shot between the fingers. These centers have in early years elaborated many such psychomotor complexes, have interrelated them, and have put them in bewildering intimacy with all the other centers in the brain and below it. Broca's convolution furnishes what is probably by far the most complex example of an eupraxic center, and its function is just the elaboration, interrelation, and extra-relation of a certain set of objects, namely, words. These, spoken, written, printed, graven, depicted, sculptured or materialized by the sign-manual constitute the physical basis, so to say, of the one uniquely human process of language-formation and language-expression. Need one try to imagine the details of Max Mueller's speech-center in terms of neur-axones, dendrites, nerve-cells, or of ions and osmosis and reversing colloids? Yet this must be done some time and by some one or else all our fashionable localization be given up.

In a way quite homologous to this, every common object

other than words must have representation in the frontal cortex, we may assume. If, then, a man does not know how to use a sextant or an ediograph or a planimeter, or a pencil or a pair of shears, it is so only because his frontal cortex has never acquired or, from disease, has lost that particular psychomotor complex. No more can be taken out than has been put in. As has been often well pointed out, the newborn child is almost wholly apraxic, although even then something in his mechanism of efficiency represents the use of his mother's breast just as it represents and provides the means of finding it in space. The cleverest adult obviously is he who has in his frontal cortex the greatest number and the greatest perfection of detail and relationship of these motor ideas (neurograms) of movements of utility.

Each one of these is properly a meaning and there is no reason whatever to suppose that each one could not be fully accounted for in neurogramic terms if we knew the exact itinerary of every forceful impulse concerned in setting them into the mental process. Proof of the falsity of this supposition is part of the burden imposed upon those who doubt it, for it is the *natural* supposition, rational in every phase of psycho-physiology. Those who doubt it simply fail to imagine the complexity of the human brain as a working machine, or the possible combinations and interactions of its three or four thousand million neurones.

President Sanford in a recent suggestive essay (62) makes explicit certain phases (the mathematical in particular) of meaning in relation to kinesthesia; and even while his arguments strive to show the limitations of sensation in this respect, they lend us distinct aid and comfort when they are properly oriented. "The concepts of physics," he says, "tend, in a word, to state all physical phenomena in terms ultimately reducible to dermal and kinesthetic experiences, and by that very fact physics is prevented from explaining anything that lies outside the field of dermal and kinesthetic experiences because of the impossibility of translating one sort of sensation into another sort. * * * * [Yet,] we can in some cases indeed transfer meanings and use symbols derived from one sense as carriers for meanings derived from another sense, as for example, when we treat geometrical relations by algebraical symbols in analytical geometry. But this is merely a transfer of the language signs, *i. e.*, of the signs of symbols, and not of the original symbols themselves. In every case the meaning must come from somewhere (*i. e.*, ultimately from some sensory experience) and the meaning then gives limits

to the symbols as if they had originated along with it." But the present writer would respectfully inquire of President Sanford (and of Wm. McDougall) if one of the most basal and universal processes of psycho-neural life be not just this particular process of 'transfer' unlimited and fusion. Normally,—that is naturally—no one sense ever is active alone, as indeed many recent researches,—*inter alia,* those of Sewall (63), Head and Holmes (38), Head and Sherren (37), Crile (12),—have testified designedly or by implication. Functional integration is all but universal. Here is suggested one of the five or six respects in which descriptive analysis has gone so far as plainly to mislead, implying a simplicity which in fact in no wise exists. The "transfer of language signs, 'only' signs of symbols" though they be, is certainly all that one need postulate for 'explaining' the elaboration of much in the way of meanings and value out of 'simple' kinesthetic and tactile experiences. Half of what the reader knows and more has come into his soul by just this process of transfer and fusion and symbolism in his brain.

But hear Doctor Sanford again about the internal kinesthesia of the body called by another name of "general and organic sensations:" "And yet even touch itself [kinesthesia] furnishes but the outer and less important part of our empirical selves. A deeper self is the self of feeling and emotion, the self that loves and hates, that strives and aspires, that enjoys and suffers, and for this another group of senses is chiefly responsible—that inner group of general or organic sensations. It is of the reverberations of excitations within the field of these inner senses that the moving part of our emotions is constituted. The loss of feeling robs us of all that part of ourselves that makes life or anything else valuable [a word of wisdom the school ma'ms need to learn]. And that part of ourselves we owe chiefly to our general and organic sensations. To sum up briefly, I may say that we get from the chief senses, singly or in coöperation, four characteristically different abstractions: From touch [kinesthesia] we get the world of space and material reality, and force acting upon us; also, from motor touch, energy, active efficiency, and freedom; from vision we get space and the world of things, though in a somewhat different way from that in which touch gives them to us; from hearing we get our symbolic machinery of thought; from the general and organic senses, our most intimate intuitions of ourselves and the basis of our emotions." Clearly there's ample room here for all one, even a partisan, need claim at present for kinesthesia

in its relation with the intelligent will:—space, material reality, force, energy, active efficiency, freedom, and "all that part of ourselves that makes life or anything else valuable." And patient research will demonstrate to the most dogmatic 'animist' (for animists all have bodies too) sooner or later the pathways, the currents, the tidal-chart, so to speak, of the neurility by which all these experiences are represented in our mortal days.

About the logical limit of present seeming aloofness from obvious nervous impulses along named tracts and nerves, is exemplified in numberless abstract concepts of relation tinged with emotional tone. All other mental processes are relatively, always relatively, simple. But even here the kinesthetic influences come more directly into the exploit than the average psychologist as yet realizes. Merely to point out the road sometimes is an acceptable service, even if we stay behind.

Wm. McDougall cogently states and trenchantly discusses a case (the telegram-argument) which may, perhaps, usefully be taken as an instance of average complexity and of more than average interest,—as one would expect from the "hands" of the Oxford psychologist for whom psychology is "the positive science of the behavior of living things." Let us quote the situation (50): "A man receives from a friend a telegram saying—'Your son is dead.' The physical agent to which the man reacts is a series of black marks on a piece of paper. The action outwardly considered as a series of bodily processes, consists, perhaps, of a sudden, total and final, cessation of all those activities that constitute the outward signs of life; or in the complete change of the whole course of the man's behavior throughout the rest of his life. And all this altered course of life, beginning, perhaps, with a series of activities that is completely novel and unprecedented in the course of his life, bears no direct relation whatever to the nature of the physical stimulus. The independence of the reaction on the nature of the physical impression is well brought out by the reflexion that the omission of a single letter, namely, the first of the series (converting the statement into 'Our son is dead') would have determined none of this long train of bodily effects, but merely the writing of a letter of condolence or the utterance of a conventional expression of regret; whereas if the telegram had been written in any one of a dozen foreign languages known to the recipient, or if the same meaning had been conveyed to him by means of a series of auditory impressions or by any one of many different possible means of communication, the resulting behavior

would have been the same in all cases in spite of the great differences between the series of sense-impressions." The constant meaning behind all these extraneous conditions McDougall supposes to constitute "the essential link in each case between the series of physical impressions and the series of physical effects." He denies absolutely any evidence that such spiritual values of meaning as that conveyed by certain little curved lines of ink on the telegram-blank can be expressed at all in neural terms.

Yet, on page 342 of the same interesting argument for 'animism' we read a paragraph that seems to imply answer to previous doubts. "We have, then, very strong grounds for maintaining that all mental retention and reproduction are conditioned in two very different ways; one of these ways, the way of motor habit and automatism and mechanical association, is adequately accounted for by the conception of the formation of neural associations by the repeated passage of the current of nervous energy between neuron and neuron, each passage leaving the track more open for subsequent passages. (Synaptic resistances lowered?) This is the only plausible and in fact seems to be the only possible conception of the way in which mental retention can be conditioned by cerebral structure or function; but the strict limitations of this mode of retention, especially the need of many repetitions of the impressions even in very simple instances of mechanical association, show that we cannot regard it as the sole or principle condition of the higher form of retention or true memory. This we see depends upon meaning; and meaning as we have seen, is just that all-important factor in mental process to which we can assign no immediate physical correlate among the brain processes,"

It is the expression "the strict limitations of this mode of retention" to which almost anyone who was really searching for concomitant somatic conditions (instead of afraid he would find them?) would naturally object. Logically it wholly begs the question and neurologically it limits knowledge and scientific explanation and presumption somewhere near the *beginning* of the nexus between psychosis and neurosis. The present writer might reasonably be classed as an 'animist' by one who affected this particular class-name, yet he certainly hopes to see started, at least, description of the homologue of every mental process in bodily terms and that without the doctrinal exclusion of any amount of interaction between the two empirical series. This seems the easy road, identified for

once with " the straight and narrow way," by which the idealist may expect scientific (psychological) satisfaction.

Memory and habit-formation and meaning symbolized to the mind by material symbols, are facts as obvious and certain as the Cartesian 'cogito.' The symbolic method of representation in the hemispherical grey is put beyond hypothesis by the cold facts of apraxia, etc., already suggested; and at once underlying and surmounting these are the neurograms, less known but certain, representing impulse, feeling, desires, character, motives of the most basal nature, broad and deep and long as humanhood itself. Combined, these three factors seem to the writer to suggest the method, at least, of the concomitance of meaning. If the kinesthetic and retinal impressions from that grievous telegram do not in the artificially strict terms of the physiological laboratory, appear as mechanic stimuli of a life-time of asthenia and misery, at any rate of change, it is assuredly because once more, as forever yet, the *complexity* of the problem is underestimated, almost overlooked,—even by the learned Oxford reader in philosophy! One seeks too much from a little given, demanding bricks to build cathedrals to the skies without the utterly indispensable straw! The defect in our psychophysiology, in other words, is quantitative and not qualitative.

It is time the traditional notion of 'traces' 'impressed in' the brain by experience immediate or otherwise were given up out of psychophysiology for good and all. Nothing at all is gained by retaining this neo-phrenological idol of the school, while on the contrary it is a cogent example of how much the antiquated make-shift concept prevents us from advancing. There *are* no 'traces' on the brain-tissue so far as anyone has been able to learn. Memory, habit, understanding, feeling, meaning, and the rest are not dependent on any kind of 'grooves' made in the living grey fabric of the great hemispheres. Eighty-five per cent of water is this intricate layer of cells and dendrites that we call the cortex; and the ceaseless activity and change, its hurrying life, exceeds that of every other portion of the organism.

But if not as material traces or vestigia how, then, can we think the mode of retention and fusion and interaction by which alone perception, imagination, conception, reason, meaning, and allied processes are thinkable facts at all? Assuredly we shall accept the alternative (it seems to the writer the exclusive alternative in view of the facts known to him) namely, of course, the conception of active kinetic strains and stresses in the " three or four thousand millions " of neurones

making up the brain. Floods of neural influence of many kinds along the separate conducting pathways, infinite variations and relations of tonus in the nerve-cells of these neurones!

And pray why not? And let Wm. McDougall and all others who could be human disembodied, tell us why there is not here sanction in plenty for every phase of intelligence, vegetative or personal, brute or human, the crudest matters of gross fact or the most subtle shades of feeling and of meaning, momentary and individual in its influence or affecting myriads of other men in *secula seculorum.* Here is *living energy,* energy in the one thing the most complicated of all things known to the human mind, so complex that it furnishes a thoroughly rational basis for even the filmy gossamers (in Huxley's famous phrase) of the soul's experience. Why refuse to a mechanism of this description,—nay, intricate beyond all present description or imagining,—any meaning however sad or overwhelming, however far and wide from the material symbols, ' stimuli,' on the paper page? What are these symbols but cues, but keys, into this marvellous maze? What matter how simple the stimuli—does not the wondrous labyrinth of living energy with its billions of pathways remain the same, and in combination, in *living,* far in excess of every finite dream—rich in possibilities beyond the dreams of even animistic avarice?

I, for one, know no complexity in mind for which an organism so integrated is not the homologue and peer.

This maze of living energy is kinetically the direct *continuum,* for the most part, of the kinesthetic flood of influences. We get here, perhaps, a-field from the definite kinesthetic pathways technically so described by the histological neurologists; but we certainly do not at all get beyond the possibility of representing psychophysic motives in terms of energy or the somatic influences in terms of motives—whichever way one prefers it stated. Implicated in every moment of the behavior of that miserable father, whether statable as actuative or inhibitory, were kinetic (or potential?) nerve-strains representing the mediate or immediate variations in some action-system (if we include therein the epithelia). Attitudes are but crystallized kinetic kinesthesiograms (if such a term be not uncoinable?). And certainly his behavior, as a series of conscious and subconscious motives actualized, was the immediate outcome continuously of attitudes whose tenor was fixed " infinite aeons ere our race began " in the instinct of complex parental love and care.

Strangely enough, some of McDougall's own theory, adopted by many and suggested (probably quite unknown to McDougall) by Claparède (11) eight years ago, helps materially in the acceptance of the full value of kinesthesia in concomitance. His theory of fatigue (49) seems to contain a point of view at least (it is probably much more—a physiological reality in some mode or other) which, made a bit intensive, would more or less belie the notions as related above of the same McDougall self-deemed an animist. This viewpoint overlooks his reservoir-idea of the central nervous system already used above, a fertile concept on which to base a theory of motivation in the terms of nervous circuits already suggested. The name reservoir-idea is all but self-explanatory; it assumes (more or less after Sherrington, Bastian, *et al.*) that the afferent or receptor side of the nervous system plus the central adjustor influences, constitute properly a reservoir of neurility on which the efferent, effector pathways and mechanisms draw one at a time (the efferent common paths of Sherrington (64)). The whole trend of kinesthesiology conducts the same way and each helps the other. Carrying this idea only a step or two further in an entirely justifiable, yes, necessary direction, we may suppose the neuronal knots, grey matter, everywhere, in the sympathetic ganglia and the cord as well as in the brain, to be surcharged with nervous tension, potential or kinetic energy, 'awaiting' only a chance to functionally discharge. Its function would cause it to discharge quite as much, perhaps, in feeling and in temperamental matters, quite as commonly or at least as normally, into cerebral mazes standing for early-acquired complexes of affective or cognitive *meanings* as into attentive consciousness seen as voluntary movement new in every case or in the habitual reflex aspects of attention-behavior (13).

Normally the actual activities of the real man or woman are arranged in well-defined functional groups. This is clearly necessary owing to the almost universal principle of habituation. These habit-groups of contractile and secretory activities are not so much muscular and epithelial groups or complexes as they are neural, for it is, of course, just the one sole business of the nervous system to bring about this very kind of adapted integration of unitary movements into activities such as make up the actual functions, just as these combined and unified in turn are an individual life. We have not far to seek nowadays in the anatomic arrangement of nervous system to find the structural basis of this habit-grouping of muscular and glandular actions. One order of them we find

based in the almost innumerable association-complexes of the brain and spinal cord; these are the so-called "higher" functions and are concerned immediately with the freer or voluntary activities. The other order of action-complexes are basally of the vegetative kind, are more or less reflexly (that is, mechanically or chemically) determined, and have their basis in the numerous ganglia and other functional groupings of the "autonomic nerves" (a necessary new name for the familiar sympathetic plus certain other similar nerves concerned with the alimentary canal). One sees in the sympathetic groups of ganglia, some of them arranged along the sides of the spinal cord; others (for example the solar plexus) huge orderly knots of protean neurones in the abdominal cavity; while still other are scattered in large numbers, but functionally related, in the various viscera of the thorax and abdomen. As is well known, it is almost the entire duty of these ganglia and of the fibers relating them, to direct the activities of the involuntary ("smooth") muscle and of the epithelium, whatever be the precise functions of the particular muscles or glands. This functional set of nerves is intimately related, in ways too complex to be here described, with the cerebro-spinal axis of nerve-paths. Moreover, it represents not only the movements but probably the normally ill-defined sensations of the tissues it supplies.

Now, because these unit-complexes of the vegetative functions of life (nutritional, circulatory, reproductive, etc.) represent the real biologic basis of the nature of the individual, we have to think of them as forming to a considerable extent the *fundamental somatic character of the man or woman as determining his or her biologic, phylogenic interests.* Thus, for example, the man with an abnormally capacious and habitually overburdened alimentary canal, that of the glutton, will have these visceral characters in some way represented in the habit-complexes of his sympathetic ganglia; in the sympathetic of the nymphomaniac the same conditions, properly adapted, must be present; and so on in all the organic systems. The unit-complexes especially of the sympathetic represent then, in some way, without a question, the basal vegetative peculiarities of the individual. It is these obviously that determine more than all else the biologic and phylenic interests and tendencies and meanings of every person as a social unit, as a link in the chain of the generations. August Hoch in a discussion of dementia precox before the American Neurological Society, succinctly stated the same group-action

of the nervous system and added another principle germane to the neurility of meaning, thus (39):

"Our memories are grouped, as it were, in more or less extensive and more or less circumscribed complexes or centers of attraction, in the formation and cohesion of which special interests take an important part. We can conceive of the mind, therefore, as made up essentially of trends of interest. In the course of individual development certain main tendencies of the personality develop which then take the lead, while other tendencies become repressed. These repressed trends exert nevertheless a marked influence on the conscious thought and activity, as Freud has shown; but in normal life they do so mainly through the fact that the energy they supply is led into profitable channels. Every trend naturally pushes toward a realization in the direction of its feelings. If this is in harmony with the main tendencies of the personality this is useful and represents the dynamic force behind our thinking and our pursuits, adapted to the environment and the given situation. If, however, trends which are not in harmony with the main tendencies of the personality and which are, therefore, under the influences of repression, no longer find an outlet in profitable channels, but assume a more or less independent dominating rôle, it is not to be supposed that the laws which govern normal mental activity should be suspended; on the contrary, we shall expect to find the same principle of the trend pushing toward its realization, while at the same time the other tendencies of the personality assert themselves in repressing influences as well as in adjustment reactions, but owing to the disturbance of balance between the usurping trend and the main tendencies of the personality, the thinking and acting is then no longer adapted to the actual situation, but appears as something strikingly out of contact with it, and is of a simpler, more crude type."

In a somewhat like way Morton Prince (58) recently has said, "Meaning is derived from and determined by past experiences. That is to say, ideas have associative relations to objects, thoughts, actions, conduct, stimuli, constellated ideas, etc., *i. e.*, past experiences represented by conserved complexes. As a result of such previous experiences various associations are built up and these complexes form the setting or context which gives ideas meaning." "In the building of complexes [*e. g.*, meanings] as we have seen, an affect becomes linked to an idea through an emotional experience. * * * * Now we must distinguish between the process which determines the

meaning of the idea and the process which determines the presence of the affect in consciousness. * * * * That which determines the meaning is, as we have seen, the setting which provides the secondary images and the associated ideas, and, therefore, the point of view. That which determines the affect is an association or linking of the whole affective mechanism (including the physiological reactions, *i. e.*, vasomotor, respiratory, secretory phenomena, etc.) to an idea. It does not give the meaning, but provides the impulsive force which tends to carry the idea to fruition. * * * * It is not a logical necessity that the original experience which occasioned the affect should always be postulated as a continuing unconscious prcoess to account for the affect in association with the idea. It is quite possible, if not extremely probable, that in the simpler types, at least, of the emotional complexes, the association between the idea and affect becomes so firmly established that the conscious idea alone without the coöperation of an unconscious process is sufficient to awake the emotion."

This application of Pavlov's discoveries is beyond cavil still more likely to work backwards, for it's a poor rule indeed that will not work both ways. Even an unconscious (actuating) kinesthetic influence coming into the "set" of the brain is readily competent to awaken a conscious idea, colored by whatever affect phylogeny and personal habit have rendered it in that particular individual. This complex serves, then, indefinitely as a motive of behavior.

Our present research into the phases of the movement-'sensations' adds a little, it may be, to this promising way of considering the nervous system's kinetic thrust, for it suggests that the duality of phase (actuating and inhibitory) found to obtain in kinesthesia proper, may be extended into the brain's grey fabric, and thereby underlie the determination of behavior —conation in its widest inclusion. Not by physiological chance, we may be sure, was the awful meaning of that well-supposed telegram asthenic and depressing to the total behavior, as a psycho-physical series, of the father, for the written symbols on the paper formed a well-established kinetic nexus with everything in his grey cortex and below bearing him thus downward. On the other hand, had the symbols meant, "You are a grandfather: ten-pound boy named for you; all well," these too would have fitted into a nexus, of the opposite tenor and effect, sthenic and stimulating, but an integration complete in itself as represented in the phylogenic and ontogenic fabric of his kinetic nature. It is not easy for the writer to understand why McDougall should venture

(deductively?) to set a perfectly arbitrary limit to the concomitance of his neurons and refuse them a share in meaning when the continuity in all respects and everywhere is so complete. Why should one require a wholly gratuitous insert ("products in consciousness of a purely psychic activity") at one particular place in a series seen to be complete without it as soon as we know our anatomy and especially our neurophysiology better than now? I have no doubt of the "products in consciousness of a purely psychic activity," but I believe them irrelevant in a series competent without them, however unified the two series be. The very essence of brain-action is synthesis and symbolism endlessly subtle and intricate, as McDougall's explicity of the reservoir-idea indeed implies. Animism *needs* no denial of the brain's subtlety and intricacy—it rests on too firm a basis and is itself too broad to require a shoring up by any destructive criticism of a series in itself utterly beyond, in its intricacy, man's present comprehension. The law of parsimony, if nothing better, would suggest the superfluity of this particular instance of interaction.

The two neural factors underlying human behavior, then, seem to be tentatively considerable as two-phased, on one hand a set of action-influences or attitudes (more or less directly related to the total compound innervation of the action-system in use at the time, and including kinesthesia proper, touch, vision, and hearing), and on the other hand, a set of kinetic fusions, involving probably in some measure of influence the whole central nervous system. These latter more especially perhaps constitute the meaning of the experience, relatively passive or active, its intent, its personal motive or purpose, all meaning being sooner or later reducible to motivation, kinetic or potential. The former, the action-set proper, is the more or less subconscious association between the more mechanical neurones of the nervous system, sympathetic, spinal, cerebellar, and cerebral. The latter (the personal, purposive, restrictive control-influences, including the resultant of the feeling-tone and of the conation proper) constitutes, in my opinion, the more conscious meaning of the organic motion represented, however remotely. Meaning as feeling plus conation plus expression-innervations, seems to underlie both the kinesthetic phases disclosed in the research, but is conscious meaning only in the inhibitory motor ideas of the implicated behavior. The meaning of that sad telegram to the father, for example, was represented in his kinetic neurograms (sympathetic, spinal, cerebral) as certain definite modes of behavior. Meaning seems irrational save on this pragmatic

motor basis, involving immediately or remotely, by the way of the neurones organic "expression" or its negation.

The majority of psychologists perhaps would suppose the correlates of meanings to be vague 'drainage'-flows between groups of central neurones acting on principals still much in debate. But whatever be the cerebral harmony of subtle action, the motive force for this meaningful "association" or fusion-process, with its "high synaptic resistances," etc., etc., is not at all apparent save on lines somewhat like those sketched above. With these tentative principles and facts in mind we shall think of meaning-fusions as a more or less direct functional continuation of the kinesthetic (*et al.*) influences flooding even the central nervous system from the action-system—the entire musculature voluntary and vegetative, and the nervous centers. The immediate meaning-innervations have, of course, an immediate independence of the current flood of kinesthetic influences, but just how much or for how long we do not as yet know. Nor do we need to know this exactly so long as we realize that the energy just behind the meaning (if we may so speak, metaphorically) comes from the muscular chemism by way of kinetic energy in muscle and nerve-cell and floods the brain with a living "head" of motivation force. The body's action as a whole, universal as tonus and more patently, keeps the great coördination-tracts tingling with uncountable thousands of kinetic impulses primarily kinesthetic.

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MAGICAL FACTORS IN THE FIRST DEVELOPMENT OF HUMAN LABOR[1]

By FELIX KRUEGER, Exchange Professor from Halle to Columbia.

The beginnings of human labor imply more of psychological problems than theoretical economy and ethnology usually admit.

We notice the common characteristic of primitive tribes, that they work or labor far less than we. I am using the word "labor" in our presently accepted sense of its meaning as a continuous, purposive and organized activity, comparatively independent of accidental stimuli, and sharply distinguished from play. Play finds immediate satisfaction in itself; while labor is always swayed by, and directed towards, more or less remote ends.

The comparatively small amount of labor engaged in by primitive peoples, obviously, is of great consequence for all early civilization. Putting aside moral considerations of leisure, egoism, and so on, this fact is usually interpreted as due to a lack of intelligence and knowledge. If primitive man, it is supposed, could know the useful consequences of a stronger and better organized form of activity, he would learn it and begin to work.

However, there are many instances, proving that primitive man, while possessing knowledge and understanding of certain kinds of economic labor, yet fails to apply himself regularly and continuously. Psychologically, he is unable to do so. On the other hand, the same men are not so absolutely unrestrained, not so shiftless and dominated only by momentary impulses, as are our tramps or criminals. Under certain conditions, we see them performing extensive and very accurately regulated kinds of work. All primitive tribes try, by complicated procedures, directly to influence the weather, the movements of the stars, birth, sickness and death, puberty, the reproduction of plants and animals in nature, and other

[1] This article is a summary of two addresses given at the recent Cleveland meeting of the American Psychological Association and before the Philosophical Club of Columbia University.

Detailed materials and more elaborate discussion will appear in a series of monographs, entitled: "Arbeiten zur Entwicklungspsychologie."

natural processes—a thing we do not attempt at all, or which we treat in a much more indirect manner.

Even where the co-operation of human activity *can* be causally very effective, as in preparing for war or the chase, in making weapons, adornments or other implements, and in the beginnings of agriculture, the care of animals or barter: the *form* itself of such activity is seldom free from very irrational factors. The more the end is subjectively important, and the less clearly its real causal relations are understood—the more primitive man's active behavior is surrounded by, and impregnated with, *magical* elements. Oftentimes a semi-rational procedure is prepared for, or followed by, a purely magical ceremony. A large part of a primitive tribe's waking life is filled with singing, music and, especially, dancing. All these—quasi-play and quasi-artistic—activities were originally more than mere amusement. First they are immediate, involuntary expressions of emotional tension. Very soon they pass into a form of conventional ritualism. Dancing and related rhythmical activities, in particular, are believed directly to effect remote ends that are subjectively represented in a more or less confused form. Dancing and music are the magical instruments *par excellence*.

An African boy was asked by a missionary why he did not work, at least as much as did his aged parents. He answered, "I dance day and night." Obviously, he implied that this activity is much more important for the whole life of his tribe, than the more rational work of women and old men could possibly be.

In many primitive languages we find one word used as a term for dancing, singing, magical ceremonies and working. In Latin "cantare" originally meant the same as "incantare." In Greek, the word for labor, "ergon," is derived from "orgia," in the sense of a rhapsodic psychophysical behavior. In semi-barbarous religions most of the gods as well as the priests are dancers, singers and musicians.

Karl Bücher collected a large amount of material, proving the rhythmical character of primitive labor. However, his genetic analysis is incomplete and his psychological interpretation is too intellectualistic. He neglects the dominating emotional factors in such phenomena, evidenced by the magico-religious apperception, in the primitive mind, of rhythm and music as well as of all those forms of activity believed to be vitally important.

When we seek in human development for the beginnings of a complicated, continuous, organized and serious activity,—

in a word,—for the beginnings of labor in the psychological sense, we find them in magical ceremonies or in those forms of regulated occupation which are centred around magical behavior and institutions.

They are the first regulated forms of social activity—regulated not only in time and space but also qualitatively. There are facts which tend to prove that, originally, man (like animals) even for eating and sleeping observed no fixed times. Magico-religious ceremonies usually occur about sunset. The more important ones, like initiation or burial ceremonies, since they are participated in by large numbers, even from distant tribes, must be decided upon weeks or months in advance. These ceremonies break the earlier custom of isolated eating (which is also—but in a more negative and primitive sense—magically conditioned). Of course, this first eating and drinking in common must be largely prepared for in advance and, to a certain extent, presupposes self-restraint and forethought. The place for such ceremonies also must be fixed and prepared. It seems that, generally speaking, the first psychologically accentuated places were the holy and dangerous ones, where a man had died or lay buried, where ancestors had performed some magically important activity, where a woman first felt the stir of foetal life, and where at set times ceremonies are to be performed.

Qualitatively, these ceremonies are better ordered and differentiated than are any other forms of primitive activity. Any change in the succession or form of such acts is believed to cause immediate illness or death; a single mistake, occurring, for instance, in one of the numerous, complicated dances of Indians of the Northwest is punished by death. The initiation-ceremonies (especially those for the boys) with their long-extended preparation, are characterized by painful and other impressive forms of treatment.

Genetically speaking, such magical and religious regulations of the life first teach self-restraint, growing independence of momentary stimuli, self-sacrifice,—all of which are essential conditions of man's *capacity* to work.

Even those members of the tribe who take no active part in a ceremony are deeply influenced by it and by many of its relations. The women and young children are usually excluded from all of the more important ritualistic performances, objects and places, because they are considered too weak to undergo the varied and terrible dangers involved. This fact increases, for the whole tribe, the emotional value

of the magico-religious performances of the men. At the same time, it constitutes a basis of the first forms of division of labor and all social organization. The women are habituated to the renouncing of a large number of pleasures and important experiences, and they are accustomed to continuously care for their young children; they also perform many semi-mystic activities pertaining to their sexual and maternal life. All of these things co-operate in giving to women a peculiar class of duties and volitional habits, such as the care of fire and water, the regular supply and preparation of food (even for the future), the first forms of pottery, textile industry, agriculture and market-barter. While such economic activities were originally the exclusive task of women, the more they become complicated, of importance for the whole tribe and intricately involved, the more they are shared in by the men. For instance, all relations to foreigners, with their political and juridical consequences, are regulated by the men, because of the semi-mystic dangers emanating from every stranger.

The political and related affairs are usually the exclusive business of the adult, unmarried initiates, who are able to fight and hunt. This group, at an early stage of cultural development becomes consolidated and differentiated from the whole tribe. From its peculiar location in the camp, evolves the "Men's-House;" originally it excluded all uninitiated and all married people. From this germ there have gradually grown up the temple and memorial hall, the place for regular dancing and music, the forum, court and King's palace, the arsenal, fortress, guest- and clubhouse. The activities taking place in the Men's-house, originally have the general characteristic of being dangerous, of vital importance for the whole tribe and therefore shot through with magical elements. In this same house we first find regulated and socially organized industry.

Every magical performance itself is believed to be dangerous in so far as it is effective and powerful. Therefore, the number of persons participating tends to be increased, in order that individual danger may be diminished. Magico-religious reactions condition the earliest careful social division and correlation of functions. On the same basis arises the peculiar role of the *old* men and women, who are better acquainted with the ceremonial rules, apperceived as concurring with the will of ancestors. The whole group of women gradually come to perform determined kinds of ritualistic

activity, even in the absence of the men, but nevertheless in functional correlations with their qualitatively different activities.

Finally, the first regular occupation or profession, which is engaged in by *single* individuals, is that of the magician, medicine-man or prophet. The political or military leader is usually the same man, or one closely related to him; at any rate, he is held to possess extraordinary magical, and consequently hereditary, powers. In an analogous manner, the differentiation of the more special, industrial professions, such as that of the blacksmith, is supported and conventionally fixed by the belief in a supernatural gift, with which such men are endowed.

These magical, and therefore dangerous, powers radiate through the apparel, the implements and every act or thing tangibly connected with these persons. And the more powerful a person, the more his substantialized magical forces survive his death, regulating the lives of the following generations.

Thus, from magico-religious qualities and "taboo" arise all primitive social privilege and every institution of group or personal property.

It is obvious that all of these relations are of utmost importance for the strengthening and deepening, for the individual as well as for the social differentiation and integration, of voluntary activity.

The confused but intensive feelings (with their habitually regulated reactions) towards the supernatural dangers and powers, effectively co-operate in liberating the will from exclusive rule by momentary impulses and instinctive mechanisms. Once escaped from this thraldom, the individual and social will continue to twine itself about an increasing number of more highly developed irrational supports. Step by step, intermediate links between momentary stimuli and human reaction become more numerous, harmonious, and structural, while the originally disconnected magical stirrings, organize and differentiate themselves into religious, aesthetic and moral feelings, into social conventions, political institutions and economic practices.

It would be a mistake to infer from these considerations that the development of civilization originally was purely magical or religious, that religion had a development distinctly previous to that of economy, social organization, artistic production, etc. On the contrary, it is a common characteristic of primitive civilization that all of these directions of human behavior are undifferentiated. However, the higher the vital

importance of an institution or form of activity is held to be, the more it is freighted with and integrated by irrational elements of a magico-religious character. These elements though originally unanalyzed from the beginning form centers of emotional stress and crystallization. From thence take their origin, numerous and ramified channels of volitional directiveness. To such facts is closely related even the evolution of the judgment and the intelligence, which is continuously supported by the evolution of emotional and volitional reactions in their totality. Every peculiar ability or knowledge is originally apperceived as being a magical power, substantialized in certain persons and related objects. The first conception of causality is that of magical necessity; and, later on, the idea of universal natural law is prepared for by the conception of the all-embracing power of the Divine.

We are unable to understand the psychological continuity of human development towards our individual and social forms of life without genetically taking into account the magical and religious reactions of primitive mind upon every impressive experience, and to every situation of vital importance.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF STANFORD UNIVERSITY

SIZE AND DISTANCE OF PROJECTION OF AN AFTER-IMAGE ON THE FIELD OF THE CLOSED EYES

By FRANK ANGELL and W. T. ROOT, JR.

A. OCCASION FOR THE EXPERIMENT

During the Fall semester of 1910, a beginner's class in experimental psychology in Stanford University had occasion to estimate the size of an after-image of a disc 1 cm. in diameter on the field of the closed lids. The results of this off-hand estimation were varied; some thought the image exceedingly small, but a fraction of an inch in diameter, others thought the image much larger than the actual disc. This led to an attempt to see if there is any law or regularity governing the apparent size of the after-image or whether it is merely a matter of individual determination. Specifically the question called for a determination of the apparent distance of the after-image from the eye and its size on the closed field of vision, i. e., the field with the closed lids.

The only references to the subject which the writers were able to unearth were an allusion by Fechner in the *Psychophysik*, harking back to an investigation on complementary colors published in 1838, and a casual remark by Hering in an early number of his *Beiträge*. Fechner's statement runs as follows:—"Schaut man eine Lichtflamme oder irgend ein farbiges Object, was ein Nachbild zu liefern im Stande ist. aus einer grössern Entfernung als der deutlichsten Sehweite an, so wird das Nachbild im geschlossenen Auge dann kleiner zu seyn scheinen, als das Object; grösser dagegen, wenn man dieses aus kleinerer Entfernung als der deutlichsten Sehweite, anschaute. Das geschlossene Auge beurtheilt also die Grössenveränderungen, welche das Nachbild je nach der Entfernung des Objects erfährt, richtiger als das offene Auge die Veränderungen in der scheinbaren Grösse des Objects selbst. Denn die Objecte scheinen uns, vermöge einer uns zur Gewohnheit gewordenen Association des Urtheils mit der Empfindungen, bei verschiedenen, nur nicht gar zu grossen. Abständen betrachtet, immer dieselbe Grösse zu behalten; obwohl natürlich ihr Bild im Auge hiebei einen verschiedenen einnimmt. Im geschlossenen Auge vermissen wir die Umstände, welche unser Urtheil hiebei leiten, und es bleibt bloss die Empfindung des Raumes übrig, den das Bild auf der Netzhaut einnimmt; wobei ein unwillkührlicher Vergleich seiner Grösse mit der Grösse stattfindet, welche das Bild bei Betrachtung des Objects aus der deutlichsten Sehweite erlangt."[1] For Fechner therefore the size of the after-image in the field of the closed eye seemed to agree with that of the inducing object when the latter was placed at the point of clearest vision, though this

[1] *Poggendorff's Annalen*, XLIV, 1838, 524.

opinion would seem to have been an *obiter dictum* springing from general impressions. At any rate there is nothing to show that Fechner had tried to work up the matter quantitatively.

Hering's observation occurs in the midst of a criticism of Wundt's projection theory of vision, and is to the effect that the size of the after-image in the field of the closed eyes depends on the distance at which one images it.[2] But this again appears to be a statement based on casual observation or perhaps on the inference that the conditions affecting the size of the after-image in the open field of vision would also govern it when the field was closed. With so skilled an observer as Hering, it is well to 'go slow' in opposing opinions in matters optical; but at any rate none of the observers in the present investigation found any noticeable change in the size of the image though imagining it placed at different distances.

B. Description of Experiment

Apparatus. After considerable experimenting as to form, size, color and distance of the object producing the after-image, the following arrangement was found most satisfactory. The inducing object was a 22 cm. square of red paper (shade No. 1, Bradley Educational Colored Papers), placed with its center at the level of the eyes on a sheet of gray (Bradley green gray No. 1) 52 cm. x 62 cm. The whole was tacked to a vertical wooden screen, the base of the screen being on a line parallel to and 71.12 cm. from the side of the table at which the observer was seated. To a second wooden screen like the first, two sheets of green gray paper (Bradley No. 1) 52 cm. x 62 cm. were tacked at the top only, allowing the upper sheet to be easily put out of the way. The under sheet was ruled in 5 mm. squares, every fifth line being heavy, making heavy lined 25 mm. squares. A meter stick fastened to the edge of the table between the observer and the first screen served as a scale along which the second screen was moved with its plane parallel to the plane of the first screen and at right angles to the line of vision of the observer. A headrest completed the apparatus. The room was lighted by two south windows behind the observer, and lying to his left and right. The light was kept as nearly uniform as possible.

The results noted here are from three observers, *A., C., L.; A.* and *C.* are members of the Psychology Department; *L.* is a student with considerable experience in laboratory method and in introspection. A fourth reagent took part in the investigation, but on account of visual troubles which made the development of after-images uncertain, the findings of this reagent are not entered in the tables.

Comparison and Projection of After-images

The observer was seated directly in front of the square of red paper, as close to the table as possible. The second screen was placed along the edge of the table in front of the observer, thus making a vertical wall in his face. By aid of the headrest the head was pushed forward until the nose almost touched the screen. The distance from the eye to the screen was measured; then the first or stimulus screen (with the square of red paper) was adjusted until the distance from the eye to the red square was exactly 71 cm. The second screen was then removed. This procedure was carefully followed from day

[2] *Beiträge zur Physiologie*, Heft 1,638.

to day, thus keeping the distance from the eye of the observer to the screens constant and the position of the head unchanged.

Securing the Distance of Projection

The following instructions were given to the observer:—"On the signal, 'Ready,' fix the gaze and attention on the center of the red square. (Indicated by a small dot of white paint. A fixation of 10 seconds was allowed.) On the signal, 'Close eyes,' do so and note the size of the after-image on the fields of the closed lids. Then project the image on the screen of gray paper which has been placed before you. Try to project the image on the surface of the screen (not through it), at the place indicated by the dot. (A dot was placed directly on a level with the eyes on the second or projection screen at the time of adjusting the headrest. The dot assisted in holding the after-image quiet and also prevented increasing the distance between eye and screen by sideward and downward projection.) Note the size of the projected image and compare size with the image on the closed lids, using the latter as a norm. Give introspections. The judgment-terms you are to use are as follows: much less, less, doubtful. like, larger, much larger. State whether the image is clear or dim."

The procedure was then as follows: The observer developed an after-image, closed his eyes and noted the size in the darkened field. Meanwhile the experimenter had placed the second screen of gray paper in front of the observer,—in the preliminary experiments the screen was placed about 65 cm. from the eye of the observer to begin with,—who then projected his image on the gray screen and compared its size with that of the image projected on the closed field of vision. The experiment was then repeated, the projection-screen being moved nearer each time until the image on the screen was pronounced smaller than the image with the lids closed. In this series a region of like and doubtful judgments was usually passed through. In a subsequent series the time and space order were reversed. When binocular vision became difficult, as the screen approached the face, one eye was used.

Practice in developing and comparing after-images was given for several days until the observer was thoroughly familiar with the whole procedure. During this time the experimenter was able to outline roughly the distance from the eye at which the images under the two conditions of projection were pronounced 'like.' Having roughly blocked in the field, the projection screen was moved back and forth 6 or 7 cm. at a time. A set of experiments consisted in securing a series of judgments from much greater to much less, passing through an intermediate series of 'like.' Ten such series of experiments were performed with each observer. With increasing practice, there came a very considerable increase in ease in comparing the two forms of after-image, so that many judgments of larger and smaller were delivered with a feeling of complete security. One of the reagents was able to make a double comparison, i. e., after comparing the image with eyes closed and the image on the gray screen he was able to close the eyes again and make the reverse comparison. Still, for obvious reasons the comparison of images is not as easy a process as comparing outside objects, so that in giving figures of the results it seems better to indicate the region within which the images may be regarded as approximately equal rather than to figure out set distances.

C. Results

The regions of fluctuating, like and doubtful judgments for the several reagents are as follows:

Table I

	Cm.	*Larger*	*Smaller*	*Like and Doubtful*
	17,8	4	3	3
	17,1	4	3	3
C.	16,5	2	4	4
	15,9	2	4	4
	15,3	2	6	2
	8,2	8	1	1
	7,6	5	3	2
A.	7,0	4	3	3
	6,4	1	3	6
	5,7	0	4	6
	5,1	0	10	0
	9,5	7	0	3
	8,9	3	0	7
	8,2	2	0	8
	7,6	3	0	7
L.	7,0	3	0	7
	6,4	3	0	7
	5,7	0	1	9
	5,1	0	3	7
	4,5	0	6	4

For *C.* therefore, there was a stretch of about 2 cm. lying between the distances of 15, 9 and 17.8 cm. from the cornea where he was unable to distinguish clearly or securely between the sizes of the two after-images; the corresponding values for *A.* are 2 cm. between the distances 5.7 and 7.6 cm. and for *L.* 2.5 cm. between 6.4 and 8.9 cm. But these figures differ widely from the distances of clearest vision for the several reagents.

Using Scheiner's method for determining the near-point of distinct vision, the following values were obtained as averages of 10 determinations:—

	Right Eye	*M. V.*	*Left Eye*	*M. V.*
C.	19,5	1,3	24,1	2,1
A.	74,9	3,1	73,3	2,1
L.	33,2	3,6	31,9	5,7

These figures indicate that the nearest point of clear vision does not determine the distance at which we seem to project the after-image in the field of the closed eyes or, what amounts to the same thing, in a darkened field; and the question arises: How far away is the image projected? Assuming that the apparent size of the image is governed, as in the case of the opened eyes, by the distance to which we project it, we can measure the size of the image projected on a screen and then compare the results with the figures which give the distances at which the two forms of image appeared approximately equal.

The next step of the experiment accordingly was to measure the size of the image projected on the gray paper ruled into 5 mm. squares previously mentioned, the inducing square of red paper being moved in or out with steps corresponding to those used in the first part of the work. This measurement, which was usually accomplished by comparing the upper or side lines of the image with the lines on the paper, was at first anything but an easy task; to hold the image bright and clear on the paper and at the same time measure the spaces covered by it was something requiring considerable practice.

In determining the dimensions of the projected after-image the ruled screen was moved in or out for *A.*, over a series of 20 steps: for *C.* and *L.* the steps were 18 and 15 respectively. From these series we shall select here only those which, in the first experiment, correspond to the region where the difference in size of the after-image in the light and dark fields was not noticeable, i.e., the region where they seemed about equal. These regions and the length of the side of the corresponding after-image are given in mm. in the following tables:

	C.	*A.*	*L.*
Region of equality	159—178	57—76	39—64
Side of after-image (av. of 10)	55— 63	25—30	20—25
M. V. of readings	2,5 2,3	2,4 2,6	2,2 1,6
Geometric projection of after-image at above distances	49— 55	18—24	12—20

We find then very considerable differences in the size of the after-images in the darkened field; but we are not prepared to say on what this difference depends. Noticeable is the difference between the apparent size of the image and its size calculated on the basis of the dimensions of the schematic eye. And in every case the observed image is larger than the geometrical projection of the retinal image. Broadly speaking, this difference increases absolutely and so of course relatively with decreasing distance of projection. Certain observations, made during the course of the investigation, may later be of value in working out the factors determining the size of the image in the darkened field. In the first place, the readings and measurements were practically alike for both eyes and for one eye. In equating the size of the two forms of after-image, series for each eye supplemented those for both eyes as given above, and the results always fell within the limits of the figures for both eyes.

Next, convergence seemed to play no important rôle in determining the size of the image or at least not a prominent rôle. Hering states that neither convergence nor accommodation plays any part at all.

One of the writers (*A.*) thought he noted the effect of convergence, but until he acquires somewhat of the *Virtuosität* in the control of the eye-muscles which Hering seems to enjoy, he prefers not to press the matter.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF VASSAR COLLEGE

XXI. The Effect of Verbal Suggestion on Judgments of the Affective Value of Colors

By Inez Powelson and M. F. Washburn

The method followed in this study may be briefly described as follows. Pieces 2.9 cm. square were cut from all of the ninety Bradley colored papers. The observer was shown these colored squares one at a time on a white background, and was asked to judge the pleasantness or unpleasantness of the color, using the numbers from 1 to 7 in the usual way. The order of the colors was determined by chance at the outset, but was adhered to thereafter during the research, so that the conditions of affective contrast should be uniform. The colors from the thirty-sixth to the fifty-fourth in the series, that is, the middle eighteen of the series, were presented with an accompanying verbal suggestion as to their affective value. The entire experiment was performed twice with the same subject, at sittings separated by an interval of several days. In the first sitting, for half of the observers, the verbal suggestions accompanying the middle eighteen colors of the series were suggestions of unpleasantness, and in the second sitting the suggestions were of pleasantness. For the other half of the group of observers this procedure was reversed, the pleasant suggestions being given at the first sitting and the unpleasant ones at the second sitting. The suggestions took the form of favorable or unfavorable adjectives pronounced by the experimenter as the color was shown. For example, when a given color was shown in the series with unpleasant suggestion it would be accompanied by the adjective 'faded;' when the same color was shown in the series with pleasant suggestion, its accompanying adjective would be 'delicate:' another color would be termed 'warm' in the series with pleasant suggestion, and 'crude' in that with unpleasant suggestion. When an observer had completed both parts of the experiment, the following values were calculated: the average affective values of the colors in the first series exclusive of the middle eighteen, that is, the colors with no accompanying suggestion in the first series; the average affective value of the colors with no accompanying suggestion in the second series; the average affective value of the colors under the influence of the unpleasant suggestions, and the average affective value of the colors under the influence of the pleasant suggestions. The usefulness of finding the average affective value of the colors unaccompanied by suggestion, in both series, lay of course in the fact that these numbers served to indicate whether the difference in the values of the colors with opposite suggestion in the two series was the effect of the suggestions, or the expression of a general tendency to lower or raise the values in the second trial. For instance, suppose that the average value assigned to the colors under

unpleasant suggestion was .5 lower than their average value under pleasant suggestion, the series with pleasant suggestions having been given first. If now the average value of the colors unaccompanied by suggestion was in the second series higher than in the first series, then it would seem probable that the lowering under unpleasant suggestion was really due to the suggestions, and not to some influence affecting suggested and unsuggested judgments alike. It is possible to obtain a numerical representation of the strength with which the suggestion acted in a positive or negative direction (obviously in some cases the effect of this direct verbal suggestion would be negative, influencing the observer toward the opposite affective accompaniment). Such numerical values may be calculated by finding the difference between the average affective values of the 'unsuggested' colors in the two series, the difference between the average values of the colors under the opposite suggestions, and subtracting these differences from each other, paying regard to signs. Thus for example: for one observer the series with unfavorable suggestions is the one given first. The average value of the 'unsuggested' colors in the first series is for this observer .38 lower than the corresponding value in the second series. The average value of the 'suggested' colors in the first series, that is, the colors under unfavorable suggestion, is 1.5 lower than the value of the colors under favorable suggestion in the second series. We may estimate the effect of the suggestion by subtracting .38, the amount of difference due to causes other than suggestion, from 1.5: it would thus be numerically represented by 1.12. In the case of another observer, to whom also the series with unfavorable suggestion was the first one presented, the value of the 'unsuggested' colors was .15 lower in the first series, and the value of the colors under unpleasant suggestion was .44 higher than that of those under pleasant suggestion. We must conclude the influence of suggestion to have been negative in this instance, with a force represented by the sum of .15 and .44; the unfavorable suggestions, so far from lowering the values of the colors they accompanied, overcame a slight tendency to lower values in the series where they occurred, and produced a total raising of .59. Of course it must be admitted that these numerical values are anything but exact measures of the suggestibility of the observers under the given conditions. On the one hand, the average tendency to raise or lower the values of the 'unsuggested' colors in one series as compared with another might not have manifested itself in the judgments made on the eighteen colors, in the middle of the series, had these been unaccompanied by suggestion. And on the other hand, other influences beside suggestion may have helped to produce the difference between the values of the colors under unfavorable and those under favorable suggestion. In comparing the results from individuals, therefore, small differences cannot be taken as having any significance.

We had thirty-five observers, all young women students. Twenty-five of these gave results indicating a positive effect of suggestion in altering the judgments of affective value. In eight cases the amount of alteration was more than 1, an amount which could hardly be due to any influence but that of suggestion. In six other cases the alteration was .50 or more. In eleven cases it was less than .50, and in six of these was negligible in amount. Ten observers gave results indicating a negative influence of suggestion, tending to alter the judgments in the direction opposite to the suggestion. In only one of

these ten cases, however, did the amount of alteration exceed 1; in two other cases it was .50 or more, and in five cases it was negligible in amount. The averages of the numerical values described above were .64 for positive suggestion and only .38 for negative suggestion. We may conclude, therefore, that direct verbal suggestion regarding the pleasantness or unpleasantness of a color has a fairly decided positive effect on the judgments of observers of the type and under the conditions found in our investigation.

A Note on the Comparative Pleasantness of Colors and Articulate Sounds. The average affective value assigned by all the observers in the above study to all the colors was 4.18. The average affective value of the nonsense syllables as estimated by the observers in the preceding study was 3.6. Colors, therefore, taking the agreeable and disagreeable together, are distinctly pleasanter than articulate sounds.

DISCUSSION

PROFESSOR DODGE'S RECENT DISCUSSION OF MENTAL WORK[1]

By F. M. URBAN

The wider field of dynamic psychology comprises the study of all the conditions of mental phenomena; but it is legitimate to define an investigation of narrower scope as the science of psychodynamics, which is the doctrine of energy transformations conditioning mental phenomena. This definition is slightly different from the one given by Lehmann, who defines psychodynamics as the exact doctrine of the quantitative influence of simultaneous or successive mental states on one another. Mental work can not be defined in mechanical terms, nor by opposing it to play. Neither is it possible to give this term a consistent introspective connotation since introspective differences of mental processes are no indication of conditioning energy transfers. The common measure of mental work is number and time as is seen from the adding test, where the number of operations performed in a minute is used as a measure. This measure is useful for practical purposes, but it is essentially non-dynamic; and it is even of restricted practical use because it must be assumed either that the output is maximum or that the effort is sustained and uniform. Feelings of strain and effort, the introspective indicators of mental work, are useful in some cases, but inconsistent and mutually contradictory in others. They are not the invariable conditions of mental work, but only its occasional by-products.

There remains the possibility of defining mental work in terms of organic metabolism as determined by calorimetric observations or by chemical analysis of the products of organic combustion. Theoretically, all nervous activity involves metabolism; and the question is whether we are able to correlate mental activity with metabolism and, consequently, to express it in thermodynamic units. The respiration calorimeter is not suited for psychological experimentation; but we may use the pulse-rate as a measure of metabolism because calorimetric experiments have shown that it is, under certain restrictions as to blood-pressure, proportional to metabolism, not only during predominantly physical work but also during long sustained mental activity. This correspondence is much closer for long than for short periods. This limits the scope of such investigations which, moreover, remain preliminary so long as they are not supplemented by calorimetric determinations. If we could experimentally eliminate all extraneous activities we might use the pulse-rate for arranging mental as well as physical activities along a general metabolic scale; and it is conceivable that these co-efficients of metabolism, expressed in thermodynamic units, may become a means of analysing the obscure conditions of mental life which introspection can not reach.

In the traditional experiments on the influence of mental phenomena on circulation, little attention is given to the initial state of the subject which is necessarily influenced by the instrumental arrangement and by the unfamiliar surroundings. The following specifications are

[1] RAYMOND DODGE. Mental Work: A Study in Psycho-dynamics. *Psychol. Rev.* XX, 1913, 1-42.

formulated for the recording instrument to be employed: 1. Legibility of every pulse-wave to at least 0.005 seconds; 2. evidence, in the curves, of changes in blood-pressure; 3. simple and inconspicuous attachment to the subject; 4. transmission to an adjoining room so that the recording devices may not become an object of the subject's attention; 5. greatest possible freedom of movement for the subject; 6. frictionless recorder; 7. comparable records in two different sittings of the same individual as well as of different individuals. The experimental problem thus defined was solved by a telephone-galvanometer-sphygmograph consisting of a telephone receiver, the circuit of which was completed by the string of a galvanometer. The movements of the steel armature, which rested on an insulating surface directly against the skin over the artery, were sufficient to induce measurable currents in the coils of the magnets. The receiver was placed over the temporal artery, which affords the best conditions for a permanent attachment; it was kept in place by an elastic band. The movements of the needle were photographed by means of Dodge's falling-plate device. The instrument gives almost complete freedom of movement to the subject; and only very violent bodily activity disturbs the record. The most serious limitation of the instrument is that the height of the pulse-waves in different records can not be directly compared because it depends on the initial distance of the armature from the magnets and on the velocity of its motion.

Physical activity is correlated with an accelerated pulse, from which it follows that physical and mental work may be equated. Measurements of their respective metabolism show that the mental processes of multiplication, reading, or learning a series of twelve familiar words involve less work than 6, 4, or 2 genuflections; they are closely approximated by the raising of a one-pound weight once every four seconds. The relatively small energy transformations of mental work are not surprising since electrical and heat products of nervous tissue in action are relatively small in comparison to analogous products of muscular action. We call attention to Dodge's treatment of the pulse-waves, which is novel. He gives what practically amounts to tables of distribution of the wave-length for the different experiments and attempts to draw conclusions from these data. He then tries to explain his results in the light of information as to the way in which the subjects behaved before and during the experiment.

The paper sets a new standard for sphymographic experiments. It shows that the subject may be given complete freedom of motion, which is absolutely essential for experiments which last some time. Keeping the subject motionless in the same position produces a strain and interferes with the normal development of the processes under observation, not to mention that in a somewhat prolonged sitting the subject may fall asleep, thereby depriving the experiment of all psychological interest. It seems that this requirement disposes of the plethysmograph and definitely settles the question in favor of the sphymograph as the instrument to be used in future investigations along this line.

It may be questioned whether Dodge's instrument furnishes the final solution of the problem. Some of his curves differ so widely from those traced by means of the Marey sphymograph that one can not let them pass unchallenged. The experimental critique by Mach and Marey has shown that the sphymographic curves are essentially correct; and any new instrument which traces them differently must remain under suspicion until evidence is forthcoming that the tracings

are correct. Past failures make Dodge's success appear very remarkable; and the most surprising thing, as a matter of fact, is that his curves are as good as they are. I am indebted to the head of one of the largest firms of instrument makers in Germany for the information that within the last two years he has refused to accept ten offers to construct electric recording devices of the telephone type because past experience led him to believe that such an instrument would not work. The mechanical reason is that vibrations of small amplitude, though they may be of even very high frequency, are transmitted correctly by the telephone; but the telephonic transmission of motions of considerable amplitude is inaccurate. It may be that Dodge just struck the happy medium in certain cases, but failed to do so in others.

Dodge's attempt to make a statistical study of the pulse-waves is equally significant; and it is my opinion that the solution of the problem can be reached only in this way. One of the most commonplace observations in plethysomographic and sphymographic experiments is that certain variations of the curve occur in a great many cases, but fail to occur in other cases, although the objective and the subjective conditions seem to be exactly alike. It is bootless to say that the conditions must have been different, because if they had been alike they would have produced the same result. Indeed, if we were in possession of all of the information necessary to distinguish between these groups of conditions, the difficulty would not exist at all, and all further argument would be futile. The problem is to find out the differences between these groups of conditions which seem to be exactly alike in so far as our knowledge goes and which, nevertheless, produce different results. It seems a rather obvious plan to apply those notions which have proved useful in the study of similar problems in psychophysics, and to eliminate these accidental variations by a statistical treatment of the results.

Such an investigation will, in all probability, require a much closer analysis of the pulse-curves than has hitherto been attempted; and it will be necessary to study not only the length and the height of the curves but also their shape. Wave-form depends upon a number of conditions which we can unravel to some extent at least. The elasticity of the arterial wall, the action of the heart, and the blood flow through the arterial system all have a definite influence on the form of the sphymographic curve and they may be determined from it. It will be necessary to study not only the single elevations but also the variations in form which appear in the course of an experiment. This requires a considerable number of measurements which could not be obtained without great difficulty from curves of the size which can be traced today; and it seems that such an investigation could not be undertaken until our technique has been so improved as to enable us to trace accurate curves of considerable size.

It is not possible to discuss fully Dodge's specifications for the ideal recording instrument, because this would open up the whole question of the method of expression. Specifications 3, 4, 5, and 7 will very likely meet with undisputed approval; number 2 might be omitted, since several physiologists believe that the pulse-curves do not show any evidence of blood-pressure. Number 1 seems to demand a degree of accuracy which really is not needed. Dodge gives his tables of distribution in intervals of 0.020 sec., which make it appear to be a waste of energy to trace curves which could be measured with an accuracy of 0.001 sec. Number 6 is only another way of insisting on

photographic registration, since this is the only recording device which is really frictionless. This advantage really amounts to very little so long as only the length of the waves is measured because an ordinary recording device would give this quantity with sufficient accuracy. It may be of some consequence if the form of the curve is taken into consideration; but even in this case one may rely on the observations of Mach and Marey that friction does not materially affect the tracing so long as it does not exceed certain limits in proportion to the impulse. Photographic registration, on the other hand, has the very serious drawback of being expensive and a little cumbersome, as is proved by the fact that Dodge took records during a very small part of the experiments only. I am inclined to substitute for number 6 the specification that the tracing must be correct with a sufficient degree of exactitude,—this to be tested by Mach's method; and for 1, that the curves be so large that they can be measured conveniently. I also favor the requirement that, for purposes of investigation, subjects highly trained in introspection should be used exclusively, for only in this case will one be able to correlate definite mental events with definite changes in the pulse-curves.

It may be doubted whether it is an improvement of the status of sphymographic experiments to link them up with the study of energy transformations. Any information as to energy transformations which take place as concomitants of mental processes certainly would be an extremely valuable addition to our knowledge; but no such information is at present available nor is it likely to be forthcoming soon, for calorimetric experimentation in psychology will be at least as difficult as plethysmographic or sphymographic analysis. One may venture to believe that the difficulties will be even greater because to the difficulties of isolating the mental processes will be added the difficulties of apportioning the proper amounts of energy to the different physiological processes going on at the same time. That this is not an imaginary but a very real difficulty is proved by the recent discussions between Lehmann, Exner and Hellpach on the notion of metabolism (as measured by the amount of carbonic acid secreted) during mental work. Dodge's hope that thermodynamic analysis may one day help us toward a better understanding of processes not accessible to introspection refers to an ideal state of knowledge, and has nothing to do with the present state of affairs. Similar hopes were expressed in regard to plethysmographic experiments; but the prophets have been singularly reticent of late.

Let us suppose, however, that all the experimental difficulties have been overcome successfully, and that we know the energy transformations corresponding to every mental process. Does that really give us a measure of mental work? The principle of the conservation of energy compels us to refer these energy transformations to the concomitant physiological processes, in which energy can be neither gained nor lost. The entire amount of energy, as determined by calorimetric measurement, is consumed by them; and no energy remains to be referred to the corresponding psychical processes. We have a complete understanding of the energy transformations which are involved in the physiological processes; but we are as far from a dynamic psychology as ever, for we can not equate mental work with physical energy, although we can correlate them. From this it follows that psychodynamics as defined by Dodge has the same limitations as the doctrine of some thirty years ago that psychology must express mental events in terms of brain-physiology.

The following interesting glimpse of individual psychology may be mentioned. On page 2 Dodge states that "actual science knows no other extension of knowledge except correlations." This is, of course, the well-known Mach-Pearson view which denies causal relations, and insists that the study of relations is the real field of science. In his paper on the "Theory and Limitations of Introspection"[1] Dodge finds fault with introspection because it "has never been able to fill out the causal relations of any fact of consciousness." The contradiction between these two statements is obvious; and it is all the more surprising because only a few months elapsed between the appearance of the two articles. It seems that the author vacillated between these two views as often happens when our ways of thinking are not yet fully adapted to a new idea. This is especially true in a case where the new view makes such large demands on the adaptibility of the subject as Pearson and Mach do when they ask us to give up the notion of causal connection and substitute for it the idea of functional dependence. A sentence on page 7 of the article under discussion might lead one to believe that transfers of energy which condition consciousness are the causal relations which Dodge had in mind when he denied that introspection could fill out the causal relations of the facts of consciousness.

Several passages of the paper are sure to arouse the antagonism of the reader who expects to find a calm argumentation. Expressions like "Our entire experimental knowledge of mental fatigue is on the yardstick basis" or "dynamic psychology has long been cast into the rôle of the ill-favored sister" smack of the sensational style of William James, which is not entirely pleasant to everybody. A patriotic plea for psychodynamics as a truly American science makes a curious impression at the beginning of a scientific article. The translation of the German *Bahnung* by facilitation does not seem correct; the term reinforcement appears to be in more general use. It is true that the questions connected with the problem of mental work have not aroused the interest they deserve; but it is misleading to mention Lehman as the only neglected worker in the field. It would have been not more than just to mention the name of Charles Henry, who has worked in this field for almost twenty years and who lately published a very remarkable book on *Sensation et énergie*. Dodge certainly fails to do justice to the views of the extreme introspectionists; and it would not be surprising if his views call forth a pointed answer from that quarter where every item of information which is not of purely introspective nature is ruled out of psychology. It seems to me that the only standpoint which equally takes both aspects of the problem into account is the one lately developed by psychophysics; its field is here defined as the study of those mental phenomena which are directly accessible to introspection, and of the processes and objects connected with them. This definition at once specifies the immediate object of psychology and provides a legitimate place for the information about correlates of mental phenomena which we can obtain from physiology and comparative psychology.

[1] This JOURNAL, XXIII., 1912. 226.

BOOK REVIEWS

Die Praxis der Konstanzmethode. By F. M. URBAN. Leipzig, Wilhelm Engelmann, 1912. pp. 26.

Hilftabellen für die Konstanzmethode. By F. M. URBAN. *Arch. f. d. ges. Psychol.*, XXIV., 1912. 236-243.

The second paper contains certain tables and an explanation of their use for the solving of the calculations required for the evaluation of data taken by the method of constant stimuli. The first paper also contains this same material, along with other considerations, and we shall leave the discussion of them until later.

The first paper, as the name indicates, is a short description of the method of constant stimuli, or more properly, of the calculations connected with that method. This is really a brief summary of all of the former papers of the author dealing with this subject. Of all the psychophysical measurement methods, we have perhaps, the best understanding of the processes, as well as the theory, of the method of constant stimuli. There have been a great many attempts in the last few years to elucidate the hypothesis upon which this method is based and also to lessen the work of calculation as much as possible. With the method in its present form, the author hopes that it can be used for clinical purposes with as great ease and greater accuracy than any of the other psychophysical measurement methods that are used to-day.

In the method of constant stimuli, a judgment is considered as a chance event dependent upon the relation of the intensity of the standard and comparison stimuli and upon the psychophysical constitution of the subject. The experimental procedure of this method aims to ascertain with what relative frequency the different judgments occur upon our various comparison stimuli. These relative frequencies are viewed as probabilities with which the judgment will occur; and this leads directly to the notion of the psychometric functions, which give the probabilities of our judgments for all intensities of the comparison stimulus. The calculations are effected either by interpolation or by assuming an appropriate analytical expression. Such an assumption has the character of a hypothesis which must have two general requisites: it must fit the nature of the experimental procedure, and it must fit the facts of experience. The so-called Φ (γ) Hypothesis fits the facts and is the basis of the method of constant stimuli. The calculation of the thresholds and their co-efficients of precision is rather cumbersome, because it requires the solution of a system of equations by the method of least squares.

Urban has found that, by adopting a scheme proposed by Wirth (*Psychophysik*, 1912, p. 213) this work can be reduced considerably by employing tables which require only the space of two pages. These are the tables spoken of as appearing in both papers, along with the directions for their use. These tables are sufficiently extended to enable one to calculate the constants h and c for a comparison series of fifteen stimuli. In using them, one merely looks up the values of the products for the observed relative frequencies of the different comparison stimuli; these products are then added, giving directly the

sums necessary for the setting up of the normal equations, which are then solved directly. The only check on the correctness of our calculations that is necessary is the assurance that we have copied our numbers accurately. The author shows that these tables may be applied only in cases where the number of judgments on our comparison stimuli are 25, 50, 100 or multiples of these, as the probabilities are calculated only to two decimal places. He also goes into a consideration of the accuracy of the final determination of the constants h and c, in view of the number of decimal places retained in our products.

The value of these tables can not be overestimated, as the experimental procedure of the method of constant stimuli has been so standardized that, it seems to the reviewer, with a little care and foresight, no case should occur where the tables cannot be applied to the results. An entire calculation for a series of seven comparison stimuli with the help of these tables will require only about 15 to 20 minutes; and with practice even this time may be shortened. Indeed, the reviewer has seen an entire calculation for such a series completed in less than 10 minutes. Such a calculation can not be effected in less than an hour with the help of an adding machine or any of the large multiplication tables; and even more time than this is required when using logarithms for obtaining these products. Furthermore, the chance of committing errors of calculation is very much reduced by the use of these tables. Thus, with their publication, it seems to the writer that the method of constant stimuli has become the most practical of any of the psychophysical measurement methods. With this method alone, the subject has no knowledge of the objective relation of the stimuli, which knowledge may cause an error of expectation. The theory underlying this method is probably better known than that for any of the others. The one argument against the use of the method of constant stimuli has been that the calculations required were lengthy and cumbersome; now, with the publication of these tables, this final difficulty has been removed.

Clark University, SAMUEL W. FERNBERGER.

The Consciousness of the Universal. By FRANCIS AVELING. London, Macmillan and Co., 1912. pp. x, 255.

Aveling has made a descriptive experimental study of the presence in consciousness of the universal idea. Such questions as the growth of meaning, the nature of abstraction, the existence of a thought element in meaning, and the relation of imagery to thought are examined; explanatory hypotheses and suggestions are added; and the book includes an historical sketch of seventy-two pages.

The experimentation was divided into two parts. In the first, Aveling attempted to arrange conditions under which meanings should be formed and become associated with nonsense words. The method consisted essentially in the systematic presentation to trained observers of ten sets of small pictures, with ten corresponding nonsense words,—the pictures representing familiar objects, such as carpenters' tools. musical intruments, etc., and each set being sufficiently homogeneous to justify its being grouped under one name. The words were then presented alone, and introspections obtained on the appearance in consciousness of the meaning which the word had acquired from its association with the pictures. The results were as follows: *1.* Four stages were distinguished in the growth of meaning, ranging from an initial stage in which the words had not yet acquired meaning, but

served merely as antecedents to the revival of a visual image of one of the pictures, usually of the one shown during the sitting at which the introspection was taken, to a final stage in which the stimulus word could hardly be discriminated from its meaning, with which latter no imagery was discovered. These four stages were often not clearly marked, any one being sometimes exaggerated, sometimes short-circuited, by an observer. *2.* Abstract, imageless mental contents are present in thinking. Visual imagery, the presence or absence of which was the most striking feature of the protocols, varied from time to time, and after a few experiments underwent changes which Aveling characterizes as 'blurring,' 'becoming vague'; yet at the same time the observers reported certain knowledge of meaning. Meaning is not a product of images alone, nor in combination; to enter into meaning, the image must be present *as* something, *i. e.,* as subsumed under a concept. The strong and growing association obtains between the word and the concept under which the pictures and their images are subsumed. Such concepts are imageless, and they are present in all meanings; they are mental elements, irreducible to any form of imagery. Their presence is indicated by the appearance in the protocols of such expressions as 'knew what it meant,' 'had idea of,' etc., absence of imagery being sometimes asserted by the observer. These imageless concepts 'must have been abstracted from the pictures with which the word was learned;' and from the fact of the existence in the percept of both sensorial and conceptual elements, Aveling argues that there are two kinds of abstraction, sensorial,—or the envisagement of a sensorial element apart from accompanying sensorial elements,—and conceptual,—or the abstraction of the 'ideational concept or imageless thought content' from the sensorial elements for which it may come to stand in consciousness. *3.* Regarding the relation of imagery to thought, Aveling advances the following hypothesis, adducing in its support excerpts from the introspections of his observers, with interpretations and arguments: The main associations, and only necessary ones, in thought processes obtain between pure concepts and the conceptual elements of images. Where images are revived, in so far as they may be regarded as purely sensorial, they are revived by reason of a conceptual element in virtue of which alone they can become present to consciousness as images. The purely sensorial elements may in many cases be considered as by-products of the conceptual elements with which they occur in consciousness. The function of imagery is to impart stability to the concept. Moreover, thought deals with things; and the presence of things in consciousness is characterized by the sensorial content. The genesis of the concept cannot be explained without sensorial implications; and the close association between the two in perception gives rise to the tendency observed for the concept to reproduce imagery.

In the second part of the experiment, Aveling investigated the functioning in judgments of the ten words with their acquired meanings. The method consisted in the presentation of the word, orally or visually, with a modifier and verb, the observer being asked to supply a predicate. By using different modifiers,—'all,' 'no,' 'the first,' etc.,—it was aimed to induce respectively three sorts of meanings.—affirmative and negative universal, and particular. The observers were asked to give an introspective description of the meaning of the subject and predicate, and especially to note whether the meaning of the subject had a general or particular reference in consciousness. Aveling found that universal meanings tend to be present as concepts, in pure form or accompanied

by imagery, while in particular meanings imagery is prominent. He expresses the belief that imagery which accompanies particular reference is more clear and vivid than that which accompanies universal meanings. Exceptions appeared, in that imagery was prominent with universal meanings in twenty per cent. of cases, and concepts were reported with individual meanings in four per cent. of cases. The first is accounted for on the ground that imagery appeared when thought was baffled, or occurred as representing pictures that would not fit the concept employed; also that concepts were overlooked by the observer in many cases where their presence is clearly to be inferred (131-132; 177, footnote). In discussing the four per cent. of cases where concepts were reported in individual meanings, Aveling maintains that the meanings were not truly particular, and that vague images were overlooked (188; 190; 254). The meaning of the subject may be accompanied by awareness of no reference to one or more pictures; by conscious reference to everything that could be included in the word; and by conscious reference to all or some of the pictures associated with the word. Such reference Aveling calls 'conceptual overknowledge;' and he regards it as a separate conceptual element, forming a fusion with the meaning content, for the reason that it is described by the observers in the same terms as those used in describing concepts,—'awareness of,' 'idea of,' etc. The meaning of the predicate, which may appear as a concept, occurs sometimes before, sometimes with, the word that expresses it. The former is more likely to be the case with universal, and the latter with particular judgments. Aveling is uncertain as to the explanation of this, but suggests that imagery, prominent in individual subjects, may 'conflue' into the predicate, and strengthen the tendency of a word-image to appear with it.

Aveling's method is in many respects an improvement upon methods of previous investigators in this field. As he himself points out, it induces the transfer of a meaning already formed to a new symbol, and not the formation of a meaning itself. In the opinion of the reviewer, Aveling's introspective findings do not justify his conclusion that the imageless concept is invariably present in the thought processes; nor, indeed, do they warrant the statement that the imageless concept exists. In many instances the author has allowed himself an unwarranted liberty in interpreting his protocols in favor of the conceptual element. For example, on pages 186 and 187, the statement appears that "We have . . . . . to record the presence of imagery noted by our observers as giving the meaning of the subject in all three forms of judgment." A few lines on, and on pages 110, 131f, and 202, we read that failure of the observers to mention concepts does not establish their absence, and that "concepts were *de facto* present and operative, but they were not reported by our observers." In dealing with the individual meanings, where imagery should have been present, but was not recorded, the author states (254): "We are quite confident" that these cases are to be explained "by the presence of kinaesthetic sensations, which we have frequently observed in ordinary life as giving 'individual' meaning to words." Again, on page 148f, in discussing his theory of concepts, Aveling remarks, among other things, in answer to the possible objection that failure to report images is no certain indication of their absence, that "the absence of mention of images in a very considerable number of the protocols . . . . . is to be taken rather as a strong indication of their absence than otherwise; since in other cases images were duly noted and reported by the same observers." If vague sensorial contents are assumed in

a few cases, why not in more? And on what grounds can an investigator allege the presence of a content, not noted in a very considerable per cent. of his introspections? Aveling nowhere describes the concept; and his treatment of it as a phenomenological content demands either its description, or proof of its unmistakable recognition by the observers as co-elemental with such factors as sensation and affection.

In many cases there seems to be evidence that the supposed concept is a product of the observer's inability at the time to analyze or even describe his mental content; the introspective descriptions are often vague and indefinite, and even self-contradictory. This appears especially in the following introspection, and those following it: "I had a distinct memory idea* of a hammer, with no image and no word, 'Goral'"—the nonsense word—"was in consciousness at the time, but did not express the hammer. They co-existed co-ordinately. The idea of hammer was localized above the stimulus word" (98-99). What can an introspector mean when he states that he has an *imageless idea* of a hammer, and adds that this imageless idea is *definitely localized in space?* It would appear that Aveling and his introspectors employ the terms 'imageless' and 'idea' in a wholly novel sense. The introspections and arguments advanced (especially 111-112) do not seem sufficient to establish the validity of the statement that images and their combinations are, of themselves, meaningless.

Again, in discussing 'conceptual overknowledge,' Aveling does not do justice to the fact that he had explicitly asked his observers to watch for particular or general reference (203). The treatment of imagery, from the point of view of its relative clearness in particular and universal meanings, is confessedly inadequate (189-190). The author apparently considers only two alternative modes of conscious representation of meaning,—more or less concrete visual or auditory imagery on the one hand, and the imageless concept, on the other (103 f); and of these two alternatives he accepts the latter. The possible significance of kinaesthetic attitudes, determining tendencies, activity consciousnesses, affective tonings, etc., is largely neglected. We must, then, conclude that additional data are needed, before the existence of the abstract, imageless concept, as postulated by Aveling, can be established.

Clark University, S. C. FISHER.

Conditioned Reflexes Excited by Visual Stimuli in the Dog Following Extirpation of the Occipital Lobes. Thesis for the degree of Doctor of Medicine. By N. K. TOROPOFF. From the physiological department of the Imperial Institute for Experimental Medicine. St. Petersburg, 1908.

Dr. Toropoff points out the lack of precise experimental data concerning the functions of the occipital lobes in higher vertebrates and the resulting divergence of views held by various physiologists. This state of things he is inclined to attribute largely to a fatal shortcoming common to all previously employed methods of investigation, namely, the circumstance that in all of them the sole criterion of the influence of a stimulus of any kind upon the nervous system of an animal is more or less complicated motor reaction. Thus in studies

* The terms 'idea,' 'ideopresentation,' are consistently used by Aveling in the sense of imageless contents of conceptual nature, *cf.* 97, 109, *et al.*

of the functions of the visual centers the effects of extirpation were judged from impairment of the ability of avoiding obstacles and from failure of the movements which are normally excited by the exhibition of attractive or fear producing objects: such results are difficult to observe and to interpret.

These considerations led the author to undertake a study of the effects of extirpation of the occipital lobes in dogs with the aid of the method that had been developed about six years previously in the laboratory of Professor Pavloff, it having been demonstrated by the many researches of Professor Pavloff's school that any act of perception may be brought into connection with salivary secretion.

The simple or direct reflex of salivary secretion which is normally excited by the presence of food or of dilute acid solutions in the mouth is utilized for the development in the experimental animal of a series of "conditioned" reflexes by a process of association training, as follows: any convenient mode of stimulation is selected, depending on the special needs and applied several times a day, being closely followed each time by the introduction into the mouth of a very dilute solution of hydrochloric acid; eventually the artificial stimulus alone suffices to excite the secretion of saliva.* Three, four, or five such conditioned reflexes can be readily developed in the average dog.

The conditioned reflexes are, of course, a cortical function, and their loss or persistence following the extirpation of certain areas furnishes a ready means of localization of cortical sensory centers.

Successful experiments were carried out on four animals. The special object in each case was to determine the ability of the animal to discern *form* (dark cross made of cardboard, appearing in front of a white screen), *movement* (four black feathers mounted on a rotating axle, a black shadow moving to and fro on a white screen brightly illuminated by means of a magic lantern), and sudden changes in *intensity of light* (turning on electric lights in a room previously darkened, or turning them out in a room previously brightly lighted).

Following the establishment of a salivary fistula, each animal was subjected to a course of training until the conditioned salivary reflexes in response to the proper visual stimuli were developed. To make possible in each instance a demonstration of the specificity of the defect of function resulting from the extirpation, a conditioned reflex in response to an auditory or a tactile stimulus was also developed. The operation of extirpation was then performed and on the animal's recovery the presence or absence of the conditioned reflexes was ascertained.

In *Animal A.* ("Volchok") a portion of each occipital lobe was removed bounded below by a horizontal line extending backward from the upper end of the Sylvian fissure, and in front by a vertical line erected at the middle of the lower boundary. In this, as in the other animals, the first effect of the operation was the disappearance of all conditioned reflexes; but after several days salivation in response to sound, light, and a moving shadow reappeared; only the ability to discern (indentify?) objects was permanently lost.

In *Animal B.* ("Castorka") a larger portion of each hemisphere was removed, the lower boundary being the same as in *Animal A.*, and the front boundary being a vertical line erected at the anterior

* For a description of the method in English and for a full bibliography (to 1909) see the article by Yerkes and Morgulis in the Psychological Bulletin, Vol. VI., No. 8.

end of the lower boundary. The results were similar to those observed in *Animal A.*

In *Animal C.* ("Gryzhka") a still larger mass of brain tissue was removed: the front boundary was the same as in *Animal B.*, but the lower boundary instead of being horizontal extended obliquely downward and backward at an angle of about 135° in relation to the vertical line. Again the conditioned light reflex returned as did the conditioned sound and tactile reflexes; the discernment both of form and of movement was lost. Further, an added phenomenon was observed, "the chaotic state:" a whole series of non-specific auditory and tactile stimuli—electric bell, pricking of any part of the skin surface, application of heat or cold—produced reflex salivation.

Finally, in *Animal D.* ("Sultan") the largest mass of brain tissue was removed: by an oblique cut starting from a point at about the middle of the great longitudinal fissure and extending downward and backward through the upper end of the Sylvian fissure fully one-third of each cerebral hemisphere was excised; in other words, not only the occipital lobes were removed but also portions of the parietal and temporo-sphenoidal lobes. All the conditioned reflexes, including those for auditory and tactile stimulation, were lost, with this exception: in the training prior to the operation the dog was grasped by the jaw whenever the acid solution had to be introduced into the mouth and thus a special conditioned reflex had been formed unintentionally, and this reflex was found after the operation to have been preserved, so that while no secretion of saliva resulted from stimulation with scratching, sound, or light, trickling at the fistula was observed regularly every time the animal's jaw was grasped.

Not the least interesting result in the entire series of experiments was a demonstration of the apparently unimpaired educability of *Animal D.* following the operation: Dr. Toropoff easily succeeded in developing a new and highly specific conditioned reflex to occur in response to stimulation with the odor of camphor.

A. J. ROSANOFF.

Stuttering and Lisping. By E. W. SCRIPTURE. New York, The Macmillan Co., 1912. pp. xiv., 251. Price $1.50.

This is a practical book, which has been prepared to meet the needs of physicians and teachers. Part i. discusses Stuttering (description and cause; symptoms, forms, nature; diagnosis; therapy; methods of treatment); Part ii., Lisping (general discussion; negligent, organic and neurotic lisping; cluttering); and Part iii. outlines 18 sets of Exercises. There are over a hundred illustrations, including a large number of graphic records. Dr. Scripture's experience in the speech-department of the Vanderbilt Clinic, as well as his theoretical work on phonetics, have well fitted him for the task he has here undertaken.

The Psychology of Insanity. By B. HART. Cambridge Manuals of Science and Literature. Cambridge, University Press; New York, G. P. Putnam's Sons, 1912. pp. ix., 176. Price 40c net.

After an outline of the history of insanity, through the demonological, political or social, physiological and psychological periods, and a brief characterisation of the psychological point of view, the author arranges his material under three heads: symptoms, classification, explanation. The keynote of classification is Dissociation, and

of explanation, Conflict. The greater part of the book is taken up with a discussion, in terms of Freud's and Jung's hypotheses, of this explanation by Conflict: successive chapters deal with complexes, repression, manifestation of repressed complexes, projection, irrationality, phantasy or day-dreaming. In a final chapter on the Significance of Conflict the writer allows great importance to the sex-instinct, but also lays emphasis on the 'herd-instinct' of Trotter.

The details are held thoroughly in hand, and the style of the work is easy and pleasant; only the continual recurrence of the didactic 'now' becomes somewhat annoying.

Ueber den Traum: experimentell-psychologische Untersuchungen. Von J. Mourly Vold. Herausgegeben von O. Klemm. Zweiter Band. Leipzig, J. A. Barth, 1912. pp. vi., 449-879. Price Mk. 11.

The first volume was reviewed in the Journal, xxii., 1911, 455 f. Enough was said at that time to indicate the importance of the work. The present, concluding installment of the material covers (1) normal, non-experimental dreams, with stimulation of the lower extremities; dream phenomena in pathological conditions of the lower extremities; (2) experiments on stimulation of the upper extremities; glove-experiments, etc.; experiments with left-handed persons; and (3) experiments on back and foot; the part played by touch and temperature sensations in dreaming; dreams composed of a number of elements (nightmare; the dream of dreaming; dream speech); the dependence of dreams on various conditions (brief muscular excitation on the evening preceding the dream; visual images; habitual dreams; inheritance of dreams). It is evident that the hoped-for theoretical discussion of the dream-consciousness is not forthcoming; we are again left with a number of somewhat discontinuous observations, valuable in themselves, but needing systematic treatment. There is still no index.

Aristoteles über die Seele. Neu übersetzt von A. Busse. Philosophische Bibliothek Bd. 4. Leipzig, F. Meiner, 1911. pp. xx., 121. Price Mk. 2.20.

Since the appearance of Kirchmann's work in 1871, we have had a number of translations of the *De Anima*: in German, those of Bender, Rolfes, Essen and others; in French, that of Rodier; in Italian, that of Razzoli; in English, those of Wallace, Hammond, Hicks. Dr. Busse, who bases his translation on the text of Biehl, though he admits a number of conjectural emendations, sets his work in direct opposition to that of Kirchmann: "bevor wir die Frage der sachlichen Richtigkeit stellen, haben wir erst die Frage nach dem richtigen Sinn zu lösen." He has utilised the results of his predecessors, but relies especially upon the Greek commentators, "die in ihrem ganzen Denken dem Verfasser soviel näher standen und deshalb seinen Gedankengängen leichter folgen konnten." A brief introduction (pp. vii.-xviii.) outlines the history of psychology from the Pythagoreans to Aristotle, and says what is necessary of the reliability of the Mss.; the text then occupies pp. 1-94; and the following notes (pp. 95-115) are explanatory of the text, or deal with salient points of textual criticism. The translation is clear and straightforward; and the cheapness of the book should give it a wide popularity.

A Brief History of Modern Philosophy. By H. HOEFFDING. Translated by C. F. Sanders. New York, The Macmillan Co., 1912. pp. x., 324. Price $1.50 net.

Professor Höffding has here compressed into 300 pages of fairly large print the history of philosophy from the beginning of the sixteenth to the end of the nineteenth century. When the German edition appeared in 1905 the reviewer read it with interest,—read it, however, rather as a summary of the writer's views and attitude than as a text-book for the use of students. It is, indeed, not easy to see what place the work has as a text; it is too full of facts, too much of a *catalogue raisonné* for the beginner, and it is too sketchy for the advanced student; possibly it will come to its own as the basis of a course of more discursive lecturing. The translation is adequate at the beginning, but grows somewhat careless as the book proceeds.

The nine 'books' deal respectively with the Philosophy of the Renaissance; with the Great Systems (Descartes, Hobbes, Spinoza, Leibniz); with English Empirical Philosophy; with the Philosophy of the Enlightenment in France and Germany; with Kant and the Critical Philosophy; with the Philosophy of Romanticism; with Positivism; with New Theories of the Problem of Being upon a Realistic Basis (modern materialism; Lotze, Hartmann, Fechner, Wundt; Bradley, Fouillée); and with New Theories of the Problems of Knowledge and of Value.

Précis d'autosuggestion volontaire; éducation pratique de la volonté. Par G. BONNET. Paris, J. Rousset, 1911. pp. iv., 302. Deuxième édition, revue et augmentée. Price fr. 3.50.

"One fact dominates the whole situation. It is the undeniable preponderance of autosuggestion in all the acts which have as intent and result the amelioration of our physical, intellectual and moral condition. Autosuggestion is everywhere; it intervenes on every occasion." Such is the text of the present work. As to the base of autosuggestion, it is to be sought in nerve-force, which is simply a mode of the universal force of electricity; "the will is a cerebral faculty by which we are able, *freely*, to dispose of a part of our nerve-force in the interest of some determinate, physical or intellectual task." The author outlines a history of hypnotism, and gives numerous instances of autosuggestion from his own experience. Then, turning to practical matters, he writes on the Education of the Will, on Self-reliance, on the Concentration of Thought, and on Personal Power. Many of the exercises recommended would have a wholesome effect; others seem to the reviewer to be distinctly questionable. The whole work is a mixture of science, common sense and a sort of mysticism, of which one can only say that it will probably do more good than harm to the uninstructed reader. We note that Dr. Bonnet accepts the experiments of Elmer Gates on the variation of color in breath-deposits with variation of emotion; Elmer Gates is "professor of psychophysics at the national *Smithson* Institute at Washington."

Mitbewegungen beim Singen, Sprechen und Hören. Von F. KRUEGER. Leipzig, Breitkopf und Härtel, 1910. pp. 22.

In this essay, which is reprinted from the *Zeits. d. Internationalen Musikgesellschaft* (xi., Heft 6 u. 7), Dr. Krüger outlines his first, provisional attitude to the Rutz theory (this JOURNAL, xxii., 1911, 450). He opens with an account of the lecture delivered by Dr. O. Rutz

at Leipzig, and of the subsequent discussion. He then marks off the problems of voice-training, medicine, historical criticism and aesthetics from the purely theoretical problem which involves physics, physiology and experimental psychology. Next he turns to the facts of observation; and he decides that both on the side of audition and on that of movement and carriage Rutz has made out a very good case; a song rendered in the 'right' type 'sounds better,' there is something alike in all renditions of the same type, the type is auditorily recognisable, the designative terms used by Rutz are well chosen, movements and changes of posture can be seen in others and felt in oneself. What, now, of the scientific setting of these facts? As regards audition, we are in presence of changes of 'tint' in the wider Helmholtzian sense, or (in the author's terminology) of a 'complex-quality.' As regards movement and posture, we have to remember the facts of sensory co-excitation, and of motor reflexes and concomitant movements; the larynx is not a musical instrument inserted into an indifferent body, but has extraordinarily varied functional and anatomical connections. The adjustments of the trunk to which Rutz has called attention are involuntary concomitant movements, not separately perceived; they may be classed psychologically as a group of expressive movements; by their effect for sensation they enter into that particular 'complex-quality' which is known as feeling.

The Social Direction of Human Evolution: an Outline of the Science of Eugenics. By W. E. KELLICOTT. New York, D. Appleton & Co., 1911. pp. xii., 249.

This little volume, based on three lectures delivered in Oberlin College in 1910, is, as the subtitle indicates, an introduction to the study of eugenics. Ch. I. discusses the sources and aims of the new science, with quotation from Galton, Pearson and others. Ch. II. reviews the biological foundations of eugenics, with elementary discussion of fluctuation and variation, Mendel's Law and the statistical phenomenon of regression. The author rightly insists that, while "millions of dollars and an incalculable amount of time are spent annually" upon endeavors to raise individuals from a lower group up to or toward the average, the benefit to society would be immeasurably greater "if the same amount of energy and money were spent in moving individuals from the middle classes on up toward the higher." That there is a positive relation between order of birth and intelligence (p. 126) seems to be settled by the recent work of Cattell (*Psych. Bulletin*, Feb. 15, 1913, p. 54: "the first-born child has the best chance to become a scientific man"). Ch. III. treats of human heredity and the eugenic programme. Many human traits are known to Mendelise, but "little can be said regarding Mendelian heredity of mental traits because the psychologist has not yet told us how to analyse even the common and simpler psychic characters into their fundamental units." A number of sample family-histories are here charted; Goddard's Kallikak family furnishes a welcome addition. As to the programme of eugenics, it consists (1) in the "extensive collection of exact data," (2) in research into differential fertility, human variability, effects of nurture, and so on, (3) in immediate practice—positive, as sterilisation, and negative, as opposition to celibacy and warfare, and (4) in "the spread of the facts, far and wide, through all classes of society."

The Sexual Life of the Child. By A. MOLL. Translated by E Paul with an Introduction by E. L. Thorndike. New York, The Macmillan Co., 1912. pp. xv., 339. Price $1.75 net.

The author, as one would expect from his previous works, has given us a monograph that is both comprehensive in scope and sane in judgment. We note a few conclusions, taken at random: "I regard as one of the gravest scandals of our present penal system the ease with which a girl who makes a pretty curtsy in the court, and who appears to be shamefaced when giving her evidence, is believed by the judge or magistrate;" "I have been forced more and more to the conclusion that . . . . . the importance of the factor of sexual experiences in the causation of disease has been greatly overestimated by Freud;" "the sexual enlightenment of the child is advisable; . . . . . but for effecting enlightment . . . . . the school is unsuitable; this matter can best be undertaken by some private person, and above all by the mother; . . . . . choice of the time . . . . . must be guided . . . . . more especially by the indications of psychosexual development;" "it has not been proved that masturbation during childhood . . . . . is generally dangerous; the possibility of danger . . . . . is, however, increased by long-continued and frequently repeated masturbation, also by the artificial postponement of the voluptuous acme, and by congenital predisposition to nervous disorders."

Professor Thorndike gives the book a strong recommendation, though he wisely adds a word of warning to those "who are unused to descriptions of symptoms of diseases, abnormalities, and defects." The meat is, indeed too strong for the average 'educated' parent; and this for the simple reason that the knowledge which Dr. Moll purveys is not counterbalanced and put in perspective by like knowledge of the other great systems and functions of the body. A very useful, and on the whole a very reassuring little volume could be made up by selection of the author's conclusions and recommendations, with omission of details.

Historical Studies in Philosophy. By E. BOUTROUX. Translated by F. Rothwell. London, Macmillan & Co., 1912. pp. xi., 336. Price $2.50 net.

The World We Live In, or Philosophy and Life in the Light of Modern Thought. By G. S. FULLERTON. New York, The Macmillan Co., 1912. pp. xi., 293. Price $1.50 net.

A First Book in Metaphysics. By W. T. MARVIN. New York, The Macmillan Co., 1912. pp. xiv., 271. Price $1.50 net.

Conduct and Its Disorders Biologically Considered. By C. A. MERCIER. London, Macmillan & Co., 1911. pp. xxiii., 377. Price $3.25 net.

We are glad to call attention to these books, though they lie somewhat far afield from the proper interest of the JOURNAL. Professor Boutroux, whose reputation in France is perhaps second only to that of M. Bergson, and who is well known in this country by his study of William James, here discourses of five great figures in the history of thought: Socrates, the founder of moral science, Aristotle, Jacob Boehme, Descartes and Kant. Professor Fullerton essays, in simple and straightforward style, "the working out of a sober realism, which will not refuse to accept suggestions from the idealist where such seem helpful, but which will take pains not to be misled into doing injustice

to the unmistakably real world given in experience." The book is meant for the plain man, and all technical notes are relegated to an appendix. Professor Marvin gives us a "student's first book in philosophy," which aims to "form a system of closely connected topics," to "represent consistently one contemporary philosophical tendency," and to accord with "the preceptorial method of instruction." After an Introduction, in which different views regarding the nature of philosophy are set forth and a definition of philosophy and metaphysics is offered, the book takes up in order the Nature of Science, and the Problems of General and of Special Metaphysics. Finally, the study of conduct, according to Dr. Mercier, resolves itself into the study of action and the study of ends or purposes. His first Book therefore examines the modes of human action under a number of headings,—spontaneous or elicited, abundant or scanty, instinctive or reasoned, original or imitative, etc.; his second Book, which is much longer, considers the ends that conduct strives to attain and the means by which these ends are compassed, dealing (always in the light of survival-value) with self-conservative and social conduct under all their manifold forms,—custom and fashion, patriotism and philanthropy, marital, parental and filial conduct, recreative and aesthetic conduct, investigation, religious conduct, etc. Dr. Mercier writes with a sincerity and vigor which compel respect, even if they do not always command assent.

Development of Religion and Thought in Ancient Egypt. By J. H. BREASTED. New York, C. Scribner's Sons, 1912. pp. xix., 379. Price $1.50 net.

This modest little book, which contains a course of lectures delivered upon the Morse Foundation at Union Theological Seminary, is a work of real importance to students of comparative religion and social psychology. Professor Breasted is known both by his field-work in Egypt and the Soudan, and by his History of Egypt; and he here gives us in broad outline his view of the growth, consolidation and decay of the Egyptian religion.

The most important body of sacred literature in Egypt is, Professor Breasted insists at the outset, not the Book of the Dead, but the older 'Pyramid Texts,'—which are, in fact, "to the study of Egyptian language and civilisation what the Vedas have been in the study of early East Indian and Aryan culture." The content of these texts is sixfold: funerary and mortuary ritual, magical charms, very ancient ritual of worship, ancient religious hymns, fragments of old myths, and prayers and petitions on behalf of the dead king. It appears from them that a court-religion, the worship of the sun-god Ra, ran parallel with the worship of the Nile-god Osiris, the deity of the common people. "The fact that both Re and Osiris appear as supreme kings of the hereafter cannot be reconciled, and such mutually irreconcilable beliefs caused the Egyptian no more discomfort than was felt by any early civilisation in the maintenance of a group of religious teachings side by side with others involving varying and totally inconsistent suppositions. Even Christianity itself has not escaped this experience." Later, in the feudal age (B. C. 2160-1788), the moral sense emerges, and social justice becomes the official doctrine of the state; these ethical ideas are, in the writer's opinion, not of Osirian but of Solar origin. Still later, Amenhotep IV. (B. C. 1383-1365) attempts, and fails, to introduce a reform of religion on a monotheistic basis. And yet later we

have the triumph of sacerdotalism; religion degenerates into usages, observances, scribal conservation of the old writings, and reaches its final decadence in the Osirianism of the Roman Empire.

There are, perhaps, two main points which may be urged in criticism of the book: first, that the Pyramid Texts are really terms in a series of documents which passes through and beyond the Book of the Dead; and, secondly, that the source and origin of the moral ideals which appear in the worship of Ra and Osiris have not been sufficiently cleared up. Professor Breasted may reply, with truth, that our knowledge of the earliest history of Egypt is still very imperfect. There is, at all events, no question as to the skill with which he has grouped his material in these lectures, and the value of the book to the student who is not expert in Egyptology.

The Lushei Kuki Clans. By LT.-COLONEL J. SHAKESPEAR. London, Macmillan & Co., 1912. pp. xxii., 250. Price $3.25 net.

The Tribes of Northern and Central Kordofan. By H. A. MACMICHAEL. Cambridge, University Press, 1912. pp. xv., 259. Price 10/6 net.

The first of these monographs, published under the orders of the Government of Eastern Bengal and Assam, describes the tribes inhabiting the hilly district which stretches, roughly, from Chittagong on the southwest to Manipur on the northeast. The population scattered over this area of some 25,000 square miles may be classed as agricultural; the tribes were originally semi-nomadic, moving their villages to clear new patches of jungle as the old clearings became infertile; but they are now settling down to permanent residence and are taking to plough cultivation. They use an interesting series of measures of length, expressed by reference to the human body; there are some sixteen or seventeen measures ranging from *chang-khat* or the distance from tip to first joint of the forefinger to *hlam* or the distance a man can stretch with both arms extended. A curious measure of weight is *chuai*, as much as can be supported if hung from the tip of the forefinger palm downwards. A mouth-organ of gourd and reeds is a ruder form of the Japanese *sho;* similar instruments, under various names, are found in Borneo; and a one-stringed bamboo fiddle is constructed like the Malagasy *valiha* or the *satong* of Sarawak, but is bowed with a bamboo strip and not plucked as a harp. A high degree of religious tolerance is shown in the sketch-map on p. 63; here a Lushai has drawn the route from his own village to the village of the dead; but the Christian's village is shown to one side, with its own road leading under the protection of Isua (Jesus) to a special Christian heaven.

The book deals in the regular way with domestic life, laws and customs, religion, folk-lore, and language of the Lushei and the non-Lushei clans, with an appendix on the families and branches of the Lushei. It is regrettable that the author uses Lushai for the inhabitants of the Lushai hills at large, and Lushei for the single clan which, under the rule of various Thangur chiefs, came into prominence in the eighteenth century: misprints are always possible, while in spoken reference the two words are indistinguishable. The volume is illustrated by water colors and photographs; the index is fairly full, but not always reliable; a map shows the localities inhabited by the several clans, and their probable place of origin.

Mr. MacMichael, whose book is comprised in the Cambridge Archaeological and Ethnological Series, deals with the tangle of tribes that dwell in northern and central Kordofan,—aiming to describe the antecedents of these tribes so far as any information on the subject can be gleaned from extraneous sources or from current native tradition. He rightly declines to accept Budge's identification of the truculent Bakkara with the Menti of the Egytian inscriptions, and of the black tribes of Sennar with the Automoloi of Herodotus; he thinks, on the other hand, that the Kuraan, a black race of Tibbu stock, may be identified with the ancient Garamantes. His work is largely a matter of balancing divergent traditions and accounts, of striking probabilities and of exploding myths; but, though he declares himself to be a mere tyro in ethnology, his book contains some useful ethnological material. It is illustrated by photographs; there is no map.

Biological Aspects of Human Problems. By C. A. HERTER. New York, The Macmillan Co., 1911. pp. xvi., 344. Price $1.50 net.

In this thoughtful and original essay, the late Dr. Herter, professor of pharmacology and therapeutics in Columbia University, has sought to approach certain problems of human life from the biological standpoint, and to interpret certain biological laws in their bearing upon human life. Bk. i. treats of the human body as a mechanism. The mechanistic theory of the living individual is accepted without reservation, though the writer declines to extend it to the 'social organism,' or indeed to press to any length the analogy between the individual and the state. The two functional powers which lie at the heart of human life are reproduction and growth, and consciousness and will. As regards the former, Dr. Herter inclines to a mnemic theory; as regards the latter, he holds that "the rational view as to the nature of consciousness is that sensory impulses, carried into an extremely elaborate cerebral mechanism, liberate there, through chemical changes in the ganglion cells, a kind of energy which manifests itself by giving to the individual the property of awareness of self;" "consciousness is a function of complex associated nervous structures in exactly the same sense that the motion of a limb is a function of complex associated neuromuscular structures." Free-will is an illusion; but scientific fatalism does not lead to hopeless resignation.

The author now passes to two instincts which "in their phylogeny or racial ancientness appear to be the most fundamental of all instinctive qualities in living protoplasm," the self-preservative instinct (Bk. ii.) and the instinct of sex (Bk. iii.). The four chapters of Bk. iii. discuss the instinct of survival, the defences of the body, self-preservation and the mental life, and death and immortality. The common interest in a future life points to a grounding in the instinct of self-preservation. "In the entire range of biological phenomena there is nothing to suggest that a continuation of life for any species is probable or necessary or desirable. . . . . I should like to observe the effects of teaching intelligent children . . . . . that a belief in personal immortality appears unreasonable and unnecessary in the light of science, and . . . . . is not improbably a . . . . . form of egotism based on the insistent obtrusiveness of the instinct of self-preservation." The three chapters of Bk. iv. are entitled Sex and the Individual, Sex and Social Relations, and the Male and Female Mind. They contain a great deal of common sense, and some heresy.

Bk. v., which was left incomplete by the author, deals with the fundamental instincts in their relation to human development; the three chapters are headed The Arts and Religion, Education and the Future of the Race, and The Fruits of Education. In these chapters there is, as is natural from their state of preparation, a falling-off from the standard of the earlier Books; given what precedes, their teaching is almost commonplace. A brief Conclusion sums up the leading ideas of the essay. There is no index.

On the whole, the volume is notable as expressing the mature views of an exceptionally intelligent and experienced man of science. That the exposition is strongest on the side of biology, weakest on that of psychology, is only what might have been expected.

Geschichte der Psychologie. Von O KLEMM. "Wissenschaft und Hypothese," Bd. viii. Leipzig und Berlin, B. G. Teubner, 1911. pp. x., 388. Price Mk. 8.

Outlines of the History of Psychology. By M. DESSOIR. Translated by D. Fisher. New York, The Macmillan Co., 1912. pp. xxix., 278. Price $1.60 net.

A History of Psychology, Ancient and Patristic. By G. S. BRETT. London, G. Allen & Co., 1912. pp. xx., 388.

The Classical Psychologists: Selections Illustrating Psychology from Anaxagoras to Wundt. Compiled by B. RAND. Boston, Houghton Mifflin Co., 1912. pp. xxi., 734. Price $3.50.

These four books, which evidence a wholesome revival of interest in the history of psychology, will be heartily welcomed by psychologists. The best of them, in the opinion of the present reviewer, is that which heads the list. Dr. Klemm, who is *privatdozent* for philosophy in the University of Leipzig and an assistant in Wundt's laboratory, has our current psychology always in mind, and his history is essentially an attempt to trace the genesis of current doctrine. Hence the plan of the book,—which begins with a section on the general tendencies of psychology, metaphysical, empirical, explanatory; continues with a discussion of the development of fundamental concepts (definition of psychology; subject-matter of psychology, consciousness; classification; the mental element; methods of psychology; mental measurement); and ends with an historical outline of the most important psychological theories (sensation, space perception, feeling, will). The emphasis on recent achievement gives the reader a sense of reality which does not often attach to historical writing; and if the perspective is radically different from that of most works on the history of thought, this is not to say that the author is mistaken in his judgment. Altogether, an excellent little book.

Professor Dessoir finds that mind has been of interest from three points of view: those of psychosophy, of psychology proper, and of psychognosis. Practical and artistic interest (psychognosis) he deals with summarily in his Introduction. The theological and metaphysical interest (psychosophy) and the biological interests which culminate in modern psychology are set forth, in strictly chronological fashion, in the body of the work; the four chapters are entitled The Ancient Conception of the Life of the Soul (from the earliest times to the patristic period), The Doctrine of the Soul in the Middle Ages and the Renaissance, Psychology of the 17th and 18th Centuries, and Psychology of Recent Times. Professor Dessoir has a keen feeling for historical

continuity; it is all the more unfortunate that his work ends with the third quarter of the last century. As they stand, however, the two histories of Klemm and Dessoir are mutually supplementary.

Professor Brett's History ends with Augustine; Siebeck takes us to Thomas Aquinas. Those who have studied their Siebeck will, perhaps, add little to their knowledge by the reading of Professor Brett's book. At the same time, Siebeck is difficult and dry; and Professor Brett is—if dry—less difficult. "The business of the historian," the author remarks, "is to record rather than interpret. He should confine himself to giving such interpretations of these phenomena as were actually given by writers contemporary with the events, and so presenting the views of both the believers and the sceptics. . . . . A history of psychology must not anticipate; it must be a record of beliefs about the soul and of the growth of the human mind in and through the development of those beliefs." That is one idea of writing a history of psychology; Klemm's is another. And it seems to the reviewer that Professor Brett, good and useful as his work is, has really chosen to fall between two stools: that of an impartial, non-anticipatory, monographic record, which, if it is to be thorough, demands far more space than he has taken; and that of a consecutive, developmental history, which demands—besides dates, biographical data, reference to cultural background—a constant prevision of the future. Let him, however, disarm criticism here as he has done in his preface. "The data included may appear to some badly selected; others will desire things that have been purposely omitted; in view of this it is permissible to indicate what method has been consciously pursued. The main emphasis is laid on what may be called psychological data in the strict sense; around these data are grouped such theories as diverge from the phenomena of consciousness to derivative doctrines of the soul's antecedents, environment, and future possibilities. The relevant parts of medical and religious theories are regarded as supplementing psychology in two different directions; the treatment of them is subordinated to psychology as the main theme." The book appears as a volume in Muirhead's *Library of Philosophy*.

A volume of selections is fair game for the critic; it is always possible to discover errors in translation, and to indicate passages that were more deserving of translation than those actually given. Dr. Rand's judgment is no more impeccable than that of another. On the whole, however, he has accomplished his self-imposed task with success, and the student who browses in his pages will make acquaintance with many an author who would otherwise remain unknown,—may (who can say?) be led by these sips and tastes to read the original writers in their entirety. It seems hardly necessary to give space in such a book to James Mill. Bain, Spencer, Lotze (Ladd's translation of the *Outlines*), Mach (Open Court translation), James, and the current translations of Wundt: are not these things in every library, at the call of the student? Yet perhaps, for the sake of historical perspective, it was worth while to include them; there is room, at any rate, for difference of opinion.

The Kallikak Family: a study in the heredity of feeble-mindedness. By H. H. Goddard. New York, The Macmillan Co., 1912. pp. xv., 121. Price $1.50 net.

Dr. Goddard has been fortunate enough, as the archaeologists say, to make a 'find;' and he has also had the training which enables him

to utilise his discovery to the utmost. Here are the facts. A family of good English blood of the middle class, settling on land purchased from the proprietors of the state in colonial times, maintains throughout four generations its reputation for probity and respectability. A scion of this fourth generation does two things: by casual intercourse with a feeble-minded girl he starts a line of mental defectives; and thereafter he marries a woman of his own quality, returns in this way to the traditions of his family, and starts a second line, of a respectability equal to that of his ancestors. Dr. Goddard has been able to follow out these two lines of descent through six generations, and thus has his finger on "a natural experiment of remarkable value to the sociologist and the student of heredity;" he is able, indeed, to make his comparisons upon far surer grounds than those on which Dr. Winship based his study of the Jukes and Edwards families. The 'good' line (Kallikak = good-bad) comprises 496 persons; here we have individuals prominent in various walks of life, while nearly all are owners of land or proprietors. The 'bad' line, coming down from the son of the original Kallikak of the fourth generation and the nameless feeble-minded girl, comprises 480 descendents; 143 of these were feeble-minded, 36 illegitimate, 33 were sexually immoral, mostly prostitutes, 24 were confirmed alcoholics, 3 were epileptics, 82 died in infancy, 3 were criminals, 8 kept houses of ill-fame, while only 46 have been found normal. Moreover, these people have married into other families, generally of about the same type; the collaterals have been traced and charted; and Dr. Goddard now has on record no less than 146 individuals.

What is the moral? "Such facts as those revealed by the Kallikak family drive us almost irresistibly to the conclusion that before we can settle our problems of criminality and pauperism and all the rest of the social problems that are taxing our time and money, the first and fundamental step should be to decide upon the mental capacity of the persons who make up these groups." Segregation and colonisation, the author thinks, "is not by any means as hopeless a plan as it may seem to these who look only at the immediate increase in the tax rate." As for sterilisation, as distinct from asexualisation, "we may, and indeed I believe must, use it as a help, as something that will contribute toward the solution, until we can get segregation thoroughly established." But, after all, "the first necessity is a careful study of the whole subject, to the end that we may know more both about the laws of inheritance and the ultimate effect of the [surgical] operation."

The book is written for the lay reader, and the strict scientific evidence for the positions taken and the conclusions drawn will be presented in a larger and more technical volume. It need not be said that this work will be eagerly expected. Meantime, the present account of the Kallikaks will do good service. The style is clear and simple: and the occasional lapses into 'journalese' will probably not offend the public to whom the monograph is primarily addressed. The well-arrayed genealogical tables, the many photographs, and the story of 'Deborah,'—a twenty-two-year-old representative of the 'bad' line, now and for the past fourteen years in the Training School at Vineland,—all add to the interest of the work for the general reader.

Experimental Psychology and Pedagogy. By R. SHULZE. Translated by Rudolph Pintner. New York, The Macmillan Co., 1912. pp. xxiv., 364. Price $3.75 net.

This book, which is said to have had a large sale in Germany, is neither a complete text-book, nor a manual of experimental psychology, nor a manual of tests. The material is drawn from a number of sources. There are two chapters dealing with the problems of measurement and the mathematical treatment of results; eight chapters describing a number of the classical experiments in experimental psychology and experimental pedagogy which the author thinks are available as tests; and a chapter each on speech, physical work, mental work, and physical correlations. The author lays no claim to completeness in any direction; he has chosen rather to present his material in a popular style; and neither the technique of the experiments nor the results are to be relied upon as standard.

However much one may sympathize with the author's plea for experimental investigation as a basis for pedagogical work, the use of many of his experiments as tests is questionable. The stimulus threshold for sound can hardly afford an index of musical talent; the method of expression is not available as a test for feelings, because psychologists are not agreed as to the correlation between organic changes and feeling; and, again, it scarcely seems worth while, from the pedagogical point of view, to determine the reaction type of the child, since there is, as yet, no correlation between the type of reactor and mental ability. Moreover the technique of many of the experiments and much of the apparatus are too difficult, both for the child and for any experimenter who is not equipped with a thorough laboratory training. On the other hand, the author's use of photography in the study of mimicry and gesture, as an objective expression of emotion, is well worth the attention of English readers. Aside from these criticisms on the pedagogical side, there are two or three errors of fact. It is an unavoidable inference that the illustrations, which the author publishes, of Traube-Hering waves and Mayer waves are sphygmograms: on the contrary they are, apparently, volumetric tracings. The statement that the Traube-Hering wave is coincident with the respiration wave is incorrect. Again, the author confuses the muscular reaction with the mechanized reaction. There is no reason for supposing that the sensory reaction might not become fully as 'mechanical' as the muscular reaction.

The book is admirably printed; there are more than three hundred illustrations,—many of them cuts of apparatus taken from trade catalogues; and there are numerous photographs of actual experiments as performed by the author. These advantages, together with the easy and non-technical exposition, make the book serviceable for the general reader who desires some knowledge of experimental investigation in psychology and pedagogy. Indeed, for this purpose, we have none better, though the reader should be warned against accepting Schulze as authoritative in so far as details in methods or results are concerned. Except for an occasional hint of German style in the English words, the work of the translator is well done. It is unfortunate that he has not cited works in English. References to Sanford, Titchener, Judd and Myers for the technique of experiments, and to Thorndike, Pearson, Whipple and Brown for mathematical treatment of results and psychical correlations, would have added to the value of the book.

H. P. WELD.

Free Will and Human Responsibility: a Philosophical Argument. By H. H. HORNE. New York, The Macmillan Co., 1912. pp. xvi., 197. Price $1.50 net.

Historically, the author tells us, mankind has passed from bondage to freedom; from custom and habit to progress and change; from determinism to indeterminism. He then reviews and rebuts nine arguments for determinism, and rehearses twelve arguments in favor of free will. So far as the intellect is concerned, free will has the better of it. But feeling and instinct must have their say; and so we turn to pragmatism, where "the wish is father to the thought," and are shown that the choice between determinism and indeterminism 'makes a difference'—to the advantage of libertarianism.

Professor Horne is arguing to a foregone conclusion, and his writing, though clear enough, is superficial. It would, indeed, not be difficult to invert his reasoning; to show that mankind has passed historically, from superstition to law, from anthropomorphism to causation; to review and rebut twelve arguments for interminism, and to rehearse nine arguments for determinism, with the result that, so far as the intellect goes, determinism has the upper hand. The appeal to pragmatism is of the nature of an appendix; it is not integral to the main thought of the book; but here, again, Schiller and Thorndike would suffice for a reversal—at the author's level—of the conclusion at which he arrives.

Die Sprache des Kindes. Von A. WRESCHNER. Zürich, O., Füssli, 1912. pp. 43. Price 80 pf.

Vergleichende Psychologie der Geschlechter. Von A. WRESCHNER. Zürich, O., Füssli, 1912. pp. 40. Price 80 pf.

The first of these booklets gives a useful summary of our knowledge regarding the appearance and development of language in the child; the exposition is based upon the works of Neumann and the Sterns, supplemented by the personal observations of the author. The second booklet reviews the experimental psychology of sex; it would be far more useful than it is had the author given references. He concludes that, in general, women excel in sensitivity, memory and feeling; men in motility, spontaneous mental activity (discrimination, power of judgment), and energy of will. He favors coeducation.

The System of the Vedânta. By P. DEUSSEN. Authorised translation by C Johnston. Chicago, Open Court Publ. Co. 1912. pp. xiii, 513.

The name of the distinguished Indologist Paul Deussen—editor of Schopenhauer, and author of the monumental *Allgemeine Geschichte der Philosophie,* four of whose six divisions are now available—is sufficient guarantee of the classical nature of the book before us. Dr. Carus has done yet another service to the contemporary world of philosophy and psychology by publishing Mr. Johnston's translation of *Das System des Vedânta;* and although that work appeared in 1883, and is long since familiar to the student of oriental philosophy, psychology and religion, we may safely predict that it will now be read by many to whom the German original has remained a sealed book. The Vedânta system has five principal parts: theology, cosmology, psychology, the doctrine of transmigration, and the teaching of liberation. Of these, the third and fourth are of greatest interest to the psychologist; and Deussen's exposition is as clear as the nature of the subject will allow: witness, *e. g.,* the summary on the interaction of body and soul, p. 341.

The Evolution of Educational Theory. By J. ADAMS. London, Macmillan & Co., Ltd.; New York, The Macmillan Co., 1912. pp. ix, 410. Price $2.75 net.

This book shows the same clearness of exposition and lightness of touch that characterised the author's *Herbartian Psychology.* It shows also a true historical perspective, a generous width of reading, and for the most part a sound critical judgment. Nevertheless, we lay it down with a feeling of disappointment. Perhaps—for the work is the first number of Sir Henry Jones' series entitled *The Schools of Philosophy: a History of the Evolution of Philosophical Thought*—perhaps we had expected something different: something more positive, more systematic, more trenchant. What we get is a balance of argument and opinion which leaves us a little bewildered by the writer's evident optimism. However, let us look at the book itself.

Ch. I is largely taken up with questions of terminology—education and instruction, teacher and pupil, educator and educand—and with a provisional definition of education as a bipolar process, of a deliberate sort, consisting in the application of personality and the communication of knowledge to a reactive personality, with a view to the modification of development. Ch. II discusses the data of education: individuality, heredity, environment, time. Ch. III, on the historical aspect of educational theory, is characteristically discursive, though it culminates in the distinction of three educational epochs, the Socratic, that of the Renaissance, and the modern. We then pass to a series of historical chapters: ch. IV opens it, a trifle paradoxically, by a discussion of the probabilities of prehistoric times. Thereupon emerges the problem which the author regards as of greatest importance for professional teachers, the problem of formal discipline as opposed to specific education. Ch. V treats of the latter in all its stages from the matter-of-course standpoint of early society down to present-day vocational training; and ch VI gives a presentation of the theory of formal discipline, of the educational organon, which historically falls between those two extremes. After this we are on familiar ground; the great theories of humanism, naturalism, idealism and mechanism are set forth in as many chapters; and the well-known names receive their due meed of criticism and appreciation. Ch. XII summarises the educational outlook, so far at least as Professor Adams' temperament allows him to summarise at all; we are to expect much from 'personal cards;' we are to give a bias towards future life-work, and perhaps in the later stages of education to cross the borderline of the definitely vocational, at the same time that we do not neglect preparation for the leisure of life; we are to improve the status and the calibre of our teachers by "the development of influences already at work;" we shall attain an educational democracy in which "all will have an education suitable to the state to which their inclinations and capacities have called them." It is all hopeful, and it all seems a little vague. But the chapters are pleasant and profitable reading, and will be useful as chapters, even if they do not cohere into a determinate 'platform.'

Principien der Metaphysik. Von BRANISLAV PETRONIEVICS. Heidelberg, Carl Winter, 1912. 570 p.

After the introduction, the author first discusses the general purpose of being and the principle of negation. Then follow the general analysis of immediate experience and the establishment of the general determination of categories of being, the world as full of and as

without relations, *sein und werden*. The second section develops negation. The first deals with the formal principles, time, space, number, motion. To this chapter there are various and elaborate appendices of a geometrical nature. The second division deals with the real categories and discusses extreme naive realism and the standpoint of metaphysical actuality. Then comes extra naive realism from the standpoint of metaphysical actuality, then absolute conscious realism or conscientialism from the metaphysical point of view; analysis of immediate experience and its basal facts, consciousness and will as the two extra conscious attributes of the ego, quantitative structure of the content of consciousness explained on the basis of the quantitative structure of the nature of will; qualitative differences, changes of the content of consciousness based on will. Then follow chapters on conscious matter, conscious soul and immortality, the dynamic world stadium and its value, negation in its relations to time, space, intensity, number, change, quality and quantity. Subsequent chapters deal with the relations of negation to the mind, thought and knowledge, assuming the indentity of thought and being, with a final chapter on the limits of knowledge in the field of original principles.

BOOK NOTES

Essais de synthèse scientifique. Par EUGENIO RIGNANO. Paris, Félix Alcan, 1912. 294 p. (Bibliothèque de philosophie contemporaine.)

The topics treated are the synthetic value of transformationism, the biologic and the energetic method, the doctrine of mnemonics, the affective tendencies, what is consciousness, religious materialism, socialism.

Reaction-time to retinal stimulation. By A. T. POFFENBERGER. New York, Science Press, 1912. 73 p. (Columbia Contributions to Philosophy and Psychology. Vol. XXI, No. 1.)

After a history of the inquiry into the speed of nerve conduction, the writer discusses its speed through nerve centers, the analysis of conduction paths, experiment on reaction time to stimulation of different retinal areas through time reactions and finally, synapsis time.

Vorschläge zur psychologischen Untersuchung primitiver Menschen. Von RICHARD THURNWALD and others. Leipzig, J. A. Barth, 1912. 124 p. (Beiheft 5, zur Zeit. f. ang. Psy.)

This publication consists of a number of articles by different people, color and space, sense, memory, apprehension, suggestibility, comprehension, counting, expressive movement, gesture and language, science and art, investigations of the mode of thought, sociology, world Anschauung. This ought to be a valuable vade mecum for the field ethnologist.

The question of association tests. By FREDERIC LYMAN WELLS. Reprinted from Psychological Review, July 1912, Vol. XIX, pp. 253-270.

This valuable monograph treats of questions of procedure, cancellation, number checking tests, addition and naming tests, formation of new associations, logical relations, the understanding of instructions, free association experiments, while the appendix has a list of 1,000 stimulus words for the latter.

Lab. Guide

Interference and adaptability. By ARTHUR JEROME CULLER. New York, Science Press, 1912. 56 p. (Archives of Philosophy, No. 24. Columbia Contributions to Philosophy and Psychology, Vol. 21, No. 2.)

The first chapter describes typewriting and discrimination reaction experiments; the next, card sorting. Then follow chapters headed individual differences, discussion and summary.

The imaginal reaction to poetry. By JUNE E. DOWNEY. Laramie, Wyoming, University of Wyoming, 1912. 56 p. (Bulletin No. 2, U. of Wyoming Department of Psychology.)

The author discusses first the characteristics of the different forms of imagery, then general characteristics of the imaginal reaction, inner speech, etc., while in the three other parts of her treatise, she treats

of the dependence of reaction upon material as seen in suggestion and style; the affective reaction, poetic fragments, with summary conclusions. She believes it possible to utilize method of style in determination of an author's type reactions and that vividness of imagery may be shown to contribute to the affective reactions to poetry.

Das Problem der objektiven Möglichkeit. Von AUGUST GALLINGER. Leipzig, J. A. Barth, 1912. 124 p. (Schriften der Gesell. f. psy. Forschung, Heft 16, iv Sammlung.)

The successive chapters here are concatenation of proof, positive and negative, the idea of cause, the ground of knowledge, cause and effect, the idea of objective possibility, empirical, regulative, concrete, abstract and hypothetical.

Ethics. By G. E. MOORE. New York, Henry Holt & Co., no date, 256 p. (No. 52, Home University Library of Modern Knowledge.)

The author treats of utilitarianism, the objectivity of moral judgments, the results of the test of right and wrong, free will, intrinsic value and concludes with notes on a few books.

Critique of impure reason. By FREDERIC LYMAN WELLS. Reprinted from Journal of Abnormal Psychology, June-July, 1912. 7 p.

Probleme der ethno-psychologischen Forschung. Von RICHARD THURNWALD. Leipzig, J. A. Barth, 1912. 33 p. (Sonderabdruck aus Beihefte z. Zeits. f. ang. Psy., Heft 5.)

A theory explaining the neural basis of subjective consciousness and the pure ego. By J. J. SEELMAN. Milwaukee, George Seelman & Sons Co., 1912. 31 p.

Lectures on moral philosophy.. By JOHN WITHERSPOON. Princeton University Press, N. J., 1912. 144 p. (Early American Philosophers.)

Association tests. By R. S. WOODWORTH and F. L. WELLS. Psychological Review Company, Princeton, N. J., 1912. 85 p. (Psychological Review Monograph, Vol. 13, No. 5, December, 1911.)

A note on the prognostic value of hallucinations in the manic-depressive psychosis. By EDMUND M. PEASE. Reprinted from American Journal of Insanity, Vol. 69, No. 1. July, 1912. pp. 119-123.

Spiritual surgery. By OLIVER HUCKEL. New York, Thomas Y. Crowell, 1912. 109 p.

Constructive eugenics. By WILLET M. HAYS. Washington, 1912. 13 p. (Reprinted from American Breeders Magazine, Nos. 1 and 2, Vol. 3.)

Die Bedeutung der Motilitätsprüfungen für objectiv-neuropsychische Studien. Von W. BECHTEREW. 1910. 40 p. (Sonderabdruck aus Folia Neurobiologica, Bd. 10.)

Mortality statistics, 1908. Washington, 1910. 705 p.

Mortality statistics, 1909. Washington, 1912. 810 p.

Studies in linguistic psychology. By ROBERT JAMES KELLOGG. Linguistic Psychology Series of the James Milliken University Bulletin, June, 1912. Vol. 1, No. 2. Published by the University at Decatur, Ill. pp. 65-128.

Das Problem der Willensfreiheit. Von G. F. LIPPS. Leipzig, B. G. Teubner, 1912. 104 p. (Aus Natur und Geisteswelt.)

The measurement of induction shocks. A manual for the quantitative use of faradic stimuli. By ERNEST G. MARTIN. New York, John Wiley & Sons, 1912. 117 p.

Folelsesbetoningens. Intellektuelle Egenvaerdi. (Er der nogen grund til en dualisme mellem erkjendeke og folelse?) By T. PARR. Kristiania, Olaf Norlis Forlag, 1912. 95 p.

English witchcraft and James the First. By GEORGE LYMAN KITTREDGE. New York, The Macmillan Co., 1912. 65 p. (From studies in the history of religions presented to Crawford Howell Toy by pupils, colleagues and others.)

Bulletin 74. Government Hospital for the Insane, Washington, D. C. Edited by William A. White, M. D. Washington, Government Printing Office, 1912. 94 p.

A dictionary of the Biloxi and Ofo languages. By JAMES OWEN DORSEY and JOHN R. SWANTON. Washington, Government Printing Office, 1912. 340 p. (Smithsonian Institution Bureau of American Ethnology, Bull. 47.)

Boletin de la Sociedad Española de Biologia. Año II, Núm. 15, Agosto, 1912. Madrid, 1912.

On the determination of alkylamines obtained from urine after kjeldahl digestion. By C. C. ERDMANN. From the Journal of Biological Chemistry, Vol. 9, No. 2, April, 1911. pp. 85-92. (From the Chemical Laboratory of the McLean Hospital, Waverly, Mass.)

Following darkness. By FORREST REID. London, Edwin Arnold, 1912. 320 p.

Psychanalysis, its theories and practical application. By A. A. BRILL. Philadelphia, W. B. Saunders Co., 1912. pp. 337.

This book certainly supplies in its way a long-felt need, for we have no attempt to state concisely the principles of the Freud psychology in English. The chief chapters are psychoneuroses, dreams, obsessions, doubts, phobias, psychanalysis, mechanism of paranoia; the psychopathology of everyday life, hysterical fancies and dreamy states, the Oedipus complex, the only or favorite child in adult life, anal eroticism and character, Freud's theory of wit. In a work of this size the author cannot, of course, go much into detail, and the work is on the whole essentially elementary and introductory. Some may think that the author has been too prone to use cases from his own experience as illustrative material, and sometimes to the exclusion of the better

material which Freud supplies, but in answer to this it can be said that a physician of experience can speak more vividly of his own, that it is natural for one who has done much translation to show a little independence, which the author's merit certainly justifies, and finally that the Brill cases are good in themselves and well selected. The intelligent reader cannot but regret, however, that more space was not given to the extremely important and suggestive departures of Jung, who seems to be breaking away from the master and evolving a set of views of his own, with which, however, Brill seems to have no great sympathy. The author is a practising physician, and readers of this journal will wish that he had given a little more attention to the psychological aspects of the subject, which just now seem to be looming up far beyond the ken of Freudians. This, however, must not detract from the great merit and serviceability of this most timely and welcome book.

Correlations of mental abilities. By BENJAMIN R. SIMPSON. Teachers College, Columbia University, Contributions to Education, No. 53. New York City, Teachers College, 1912. pp. 122.

After describing the general methods of investigation, the author describes the administration of the tests in detail, their order, instructions given in each variety and individual differences in the power to interpret; then comes the scoring of results, reliability, significance of tests, and analysis of general intelligence as shown by the difference between the good and the poor groups. Especially interesting are the tests of memory, association, apperception, motor control, test in selective thinking. Best is the comparison of these results with those obtained by other investigators.

The dynamic foundation of knowledge. By ALEXANDER PHILIP. London, Kegan Paul, Trench, Trübner & Co. Ltd., 1913. pp. 318.

Among the 38 chapters which constitute this book, which aims to present a new dynamical theory of matter, which owes something to the late J. B. Stallo, the author discusses the sensible world, the affirmative judgment, necessary postulates, activity, origins of metaphysics, realism, potential and actual, middle ages, terms of nature, sensationalism and intellectualism, defects of idealism and realism, nature of knowledge, cause, reason, materiality, necessity, energy, space, matter, axioms, science and reality, nature and art, ethics, science of language, meanings, unity of knowledge, opinion, general and singular, finally the applications of his dynamic theory to economics, education, and metrical standards.

Die Realisierung; ein Beitrag zur Grundlegung der Realwissenschaften. Von OSWALD KÜLPE. Erster Band. Leipzig, S. Hirzel, 1912. pp. 257.

This book constitutes an important pronunciamento of its author although it is only the first volume. In it he first discusses the permissibility of general realization, under which he treats the proof of consensualism, the logical difficulties of transcendence, the actual presentation of objects in consciousness; and in a second chapter he considers the proving of objective idealism. When the author has printed his work completely we hope for a fuller review.

Ästhetik der Gegenwart. Von E. MEUMANN. 2d ed. Leipzig, Quelle & Meyer, 1912. pp. 180.

The author gives an historical basis and then describes the foundations of empirical aesthetics by Fechner, whose influence he traces through another chapter. He then traces the chief direction in which the problems of present-day aesthetics have developed, defines the domain of research, the psychology of aesthetic recreation and enjoyment, the aesthetic aspect of art and of nature. A good compend with atrocious print.

Über den Willensakt und das Temperament; eine experimentelle Untersuchung. Von NARZISS ACH. Leipzig, Quelle & Meyer, 1910. pp. 324.

After an introduction in which the author treats of the object of his experiment and its limitations, describing systematic experimental self-observation in its relations to investigations of the will, the second chapter treats of his combined process and its application, the technical apparatus, the experiment, its results and the various persons taking part in the experiment. Then come the phenomenological results for the different series and results. The third chapter is devoted to the inferences from the data thus acquired concerning the act of will itself, and then the deed it does, results and applications, errors, variations, the abbreviated, weak and trained will. Chapter four is devoted to feeling and temperament.

Umriss einer neuen analytischen Psychologie und ihr Verhältnis zur empirischen Psychologie. Von WALTHER SCHMIED-KOWARZIK. Leipzig, Johann Ambrosius Barth, 1912. pp. 318.

In the first part the author discusses the essence of analysis, the empirical and analytic type of knowledge and psychology. The second part is entitled psychological systematics and treats of consciousness as a whole (the ego and the now), historical systems of the content of consciousness, various kinds of content and their divisions, and then takes up the three kinds of content which the author calls sensation, feeling and *Strebung.*

Zur Analyse der Vorstellungen und ihrer Gesetze; eine experimentelle Untersuchung. Von K. KOFFKA. Leipzig, Quelle & Meyer, 1912. pp. 392.

After characterizing the difference of the idea of description and function and the problem and methods, he gives a series of experimental tests in qualitative and quantitative analysis, also completion tests with optical stimuli, with restricted reproduction, and then passes to *Vorstellungen,* visual, acoustic, etc., and finally discusses determination, association, latent *Einstellung,* associative factors, the understanding of words, etc., on the basis of his experiments.

Experimental studies of mental defectives; a critique of the Binet-Simon tests and a contribution to the psychology of epilepsy. By J. E. WALLACE WALLIN. Baltimore, Warwick & York, 1912. pp. 155.

The more we discover of the psychology of the epileptic, the more successful will be our educational methods. The results of this book, says Whipple in his introduction, have added to our knowledge of the Binet-Simon tests. This work shows that these are far from being

simple. Moreover they have many imperfections and limitations, so that this monograph makes a valuable contribution to the critique of the tests. They were made in the New Jersey State Village for Epileptics at Skillman. The author treats at length of the variations in mental and physical tests in relation to age and gives a practical guide for the administration of the scale in measuring intelligence. He has limited himself to a purely experimental and empirical exposition, assuming that the facts tell their own story and leaving the reader to work out their amplifications as well as to draw his own conclusions from the facts supplied.

Poètes et névrosés. Par ARVÈDE BARINE. Paris, Hachette et Cie, 1908. pp. 362. 2nd edition.

This interesting book consists of a critico-psychological analysis of the works of four writers, Hoffmann, Quincey, Edgar Poe, G. de Nerval, all of whom are believed to be neurotic. The author seeks by careful study first of their life, then of their works, to define the direction and extent of their alienation and also to consider whether and if so, what, it contributed to their literary success.

L'Année Psychologique. Paris, Masson et Cie, 1912. 18th volume. pp. 525.

This number is marked by a memoir and picture of the late A. Binet. It also contains a number of articles from his pen. The volume is larger than usual and possibly more interesting.

Autokinetic sensations. By HENRY FOSTER ADAMS. Psychological Review Publication, July 1912. pp. 45.

His experimental results touch the following subjects: white light one cm. square, comparison of square and perpendicular lights, horizontal and oblique lights, McAllister's figures, the effect of size of light, monocular and binocular vision, voluntary control, suggestion, effect of light background, explanation, historical review, bibliography.

Selected papers on hysteria and other psychoneuroses. By SIGMUND FREUD. Authorized translation by A. A. Brill. Second, enlarged edition. New York, The Jour. of Nervous and Mental Disease Pub. Co., 1912., pp. 215.

The chief papers here translated are the psychic mechanism of hysterical phenomena, the cases of Lucy R. and Elizabeth v. R., the psychotherapy of hysteria, the defense neuro-psychoses, the rôle of sexuality, "wild" psychoanalysis, hysterical fancies and bisexuality.

Einführung in die Psychologie. Von ADOLPH DYROFF. 2d ed. Leipzig, Quelle & Meyer, 1912. pp. 144.

This booklet begins with the problems and aids, then treats psychic life in general, sense life, the life of concept, thought and speech, feeling and instinct, will and freedom, attention and apperception. There are no cuts, the print is fine and abominable, the literature at the end of each chapter copious and well-selected.

Questions of the day in philosophy and psychology. By HERBERT LESLIE STEWART. New York, Longmans, Green, & Co., n. d. pp. 284.

The author treats reforms in psychology, the sub-conscious, interpretation of genius, growth of public opinion, pragmatism, recidivism, pessimism, value of judgment, the cult of Nietzsche.

Über das Studium der Individualität. Von A. LASURSKI. Pädagogische Monographien, hrsg. v. E. Meumann, XIV. Band. Lpz., Nemnich, 1912. pp. 191.

This covers much the same ground as Stern's Differential Psychology. It begins by considering the idea of inclination or *Neigung* and then treats the neuropsychic organization and what are the essential bases of characterology, and how its various analyses are made, and concludes by defining the relations between individual and general psychology.

Problems in eugenics. Papers communicated to the First International Eugenics Congress, held at the University of London, July 24th to 30th, 1912. London, Published by the Eugenics Education Society (6, York Buildings, Adelphi, W. C.), 1912. pp. 490.

The papers here printed are classified as follows: 8 under biology and eugenics; 5. practical eugenics; 2 under education and eugenics. The largest divisions are sociology and eugenics, with 9 papers, and medicine and eugenics with 8.

L'Année Pédagogique, publiee par L. CELLÉRIER et L. DUGAS. First year, 1911. Paris, Félix Alcan, 1912. pp. 487.

The articles are on School and Life, by E. Boutroux; Ideal and Education, by Cellérier; Sympathy in Education, by L. Dugas; Psychological Study of the Methods of Teaching, by L. Cellérier; Primary Teaching, by X; and a bibliography for 1911.

Les maladies de l'énergie; les asthénies générales, épuisements—insuffisances—inhibitions—clinique—therapeutique. Par ALBERT DESCHAMPS. Paris, Alcan, 1909. pp. 496.

This work treats pathological defects of vigor; the first part is clinical and treats of the sources of energy, definition, etiology, symptomatology, thermic symptoms as distinct from chemical and chemicophysical, symptoms of sleep, varieties of aesthesia, pathogeny. The second part deals with therapeutics, general ideas, rest, isolation, aerotherapy, alimentation, blood pressure, remineralization, medication, etc.

Geschichte der Psychiatrie. Von TH. KIRCHHOFF. *Allgemeine Therapie der Psychosen.* Von A. GROS. Leipzig, Franz Deuticke, 1912. pp. 208. (Handbuch der Psychiatrie, hrsg. von G. Aschaffenburg. Allgemeiner Teil. 4. Abteilung.)

The historical part includes only 48 pages. In the second part the more interesting topics treated are prophylaxis and psychotherapy. In one subdivision of the latter subject its relations to religion and philosophy are treated, particularly determinism and responsibility. The author is a great believer in the therapy of occupation.

The influence of caffein on mental and motor efficiency. By H. L. HOLLINGWORTH. New York, The Science Press, 1912. pp. 166. Archives of Psychology, ed. by R. S. Woodworth. No. 22, April, 1912. Columbia Contributions to Phil. and Psychol., Vol. XX, No. 4.

Perhaps the best result of this interesting series of experiments is the complete absence of any trace of secondary depression or of sensory reaction consequent upon the stimulation, this result being of course quite in contrast with the secondary reactions believed to follow the use of such a drug as strychnine.

The history and status of psychology in the United States. By CHRISTIAN A. RUCKMICH. Reprinted from the American Journal of Psychology, October, 1912, Vol. XXIII, pp. 517-531.

Plant response as a means of physiological investigation. By JAGADIS CHUNDER BOSE. New York, Longmans, Green & Co., 1906. pp. 781.

Die Deizbewegungen der Pflanzen. Von E. G. PRINGSHEIM. Berlin, Julius Springer, 1912. pp. 326.

Race Improvement. By LA REINE HELEN BAKER. New York, Dodd, Mead & Co., 1912. pp. 137.

Die Projektionsmethode und die Lokalisation visueller und anderer Vorstellungsbilder. Von LILLIEN J. MARTIN. Leipzig, J. A. Barth, 1912. pp. 231.

Berkeley. Versuch einer neuen Theorie der Gesichtswahrnehmung und Die Theorie der Gesichtswahrnehmung verteidigt und erläutert. Tr. by Raymund Schmidt. Ed. by Paul Barth. Leipzig, Verlag von Felix Meiner, 1912. pp. 152.

Über die Tonmalerei. Von PAUL MIES. Sonderabdruck aus "Zeitschrift für Ästhetik und allgemeine Kunstwissenschaft," hrsg. von Max Dessoir, VII. Band, 4. Heft. Stuttgart, Ferdinand Enke. pp. 578-816.

Über die Tonmalerei. Von PAUL MIES. Inaugural-Dissertation zur Erlangung der Doktorwürde, Bonn, 1912. Stuttgart, Druck der Union Deutsche Verlagsgesellschaft, 1912. pp. 55.

Über eine Unruheerscheinung: die Halluzination des Anrufes mit dem eigenen Namen (ohne und mit Beachtungswahn). Von MAX LÖWY. Separatabdruck aus den "Jahrbüchern für Psychiatrie und Neurologie," XXXIII. Band. Leipzig, Franz Deuticke, 1911. pp. 131.

Grundlagen einer organischen Weltanschauung. Von M. KREWER. Berlin, Leonhard Simion Nf., 1912. pp. 73.

New phrenology. By SHEPHERD IVORY FRANZ. Reprinted from Science, N. S. Vol. XXXV., No. 896, pp. 321-328, March 1, 1912.

Zur Kasuistik der Brunnenkrise (des Brunnendusels, des Brunnen und Baderausches). Von MAX LÖWY. Sonderabdruck aus der Zeitschrift für Balneologie, Klimatologie und Kurort-Hygiene, IV. Jahrgang, n. 12. Berlin SW 48, Allgemeine Medizinische Verlagsanstalt. 1911-1912. pp. 7.

The function of the vibrissae in the behavior of the white rat. By STELLA BURNHAM VINCENT. Behavior Monographs, v. I, no. 5, 1912. Cambridge, Mass., Henry Holt & Co. pp. 81.

Zur Kasuistik seltener "dyshumoraler" (innersekretorischer) Störungen. Von MAX LÖWY. Sonderabdruck aus der "Prager Mediz. Wochenschrift," XXXVI., Nr. 34-37, 1911. pp. 37.

Experimental oral euthenics: an attempt objectively to measure the relation between community mouth hygiene and the intellectual efficiency and educational progress of elementary school children. By J. E. WALLACE WALLIN. Reprinted from the Dental Cosmos, issues for April and May, 1912. pp. 32.

Eight months of psycho-clinical research at the New Jersey State Village for Epileptics, with some results from the Binet-Simon testing. By J. E. WALLACE WALLIN. Sonderabdruck aus der Zeitschrift, "Epilepsia," Vol. III. Leipzig, Johann Ambrosius Barth, pp. 366-380.

The present status of the Binet-Simon graded tests of intelligence. By J. E. WALLACE WALLIN. Reprint from the Alienist and Neurologist, Vol. 33, No.2, May, 1912, St. Louis. pp. 14.

Southern Medical Journal. (Journal of the Southern Medical Association.) December, 1912. Vol. V., No. 11. Southern Medical Journal, Suite 905 Van Antwerp Bldg., Mobile, Ala. pp. 732-806.

Psychologie und Erziehung: Ansprachen an Lehrer. Von WILLIAM JAMES. Aus dem Englischen von Dr. Friedrich Kiesow. 3d ed. Leipzig, Wilhelm Engelmann, 1912. pp. 134.

Die Situation auf dem psychologischen Arbeitsfeld. Von REINHOLD GEIJER. Berlin, Leonhard Simion Nf., 1912. pp. 90.

Archiv für systematische Philosophie, hrsg. von LUDWIG STEIN. Neue Folge der Philosophischen Monatshefte, XVIII. Band. Heft. 4. Nov. 15, 1912. Berlin, Leonhard Simion Nf., 1912. pp. 367-488.

Myxödem und Kretinismus. Von J. WAGNER v. JAUREGG. Lpz., Deuticke, 1912. pp. 91. (Handbuch der Psychiatrie, hrsg. von G. Aschaffenburg. Spezieller Teil, 2. Abteilung, 1. Hälfte.)